CHARACTER AND PERSONALITY

An International Quarterly for
Psychodiagnostics and Allied Studies

Vol. I MARCH, 1933 No. 3

CONTENTS

ERNST KRETSCHMER, University, Marburg:
"The Experimental Method Treated as an Instrument of Psychological Investigation" 175

HANS DRIESCH, University, Leipzig:
"The Experiment: Is It the Only, and Is It a Reliable Way to Establish Psychological Facts?" 181

WILLIAM MCDOUGALL, Duke University:
"Experimental Psychology and Psychological Experiment" 195

A. A. ROBACK, Cambridge (Mass.):
"Personality Tests—Whither?" 214

ELISABETH ENKE, Neuropathic Clinic, University of Marburg:
"The Affectivity of Kretschmer's Constitution Types as Revealed in Psycho-Galvanic Experiments" 225

MARY AUSTIN, Santa Fe, New Mexico:
"Character and Personality Among American Indians" 234

E. LANDAU, Lausanne:
"Morphology and Character" 238

News and Notes:
Germany 242
Scandinavia 243

Book Reviews 245

Issued on the first of September, December, March and June

Contributions should be submitted to the Editor, Robert Saudek, 19 Corringham Rd., Golders Green, London, N.W. 11 (England), or to the Associate Editor, Ernest Seeman, Durham, N. C. (U.S.A.)

Reprints should be ordered when proof is returned. Twenty-five copies are supplied to authors free; additional copies at cost. Correspondence concerning editorial matters should be sent to the Associate Editor; that concerning subscriptions, change of address, and back numbers to The Duke University Press.

Subscription price, $2.00 per year ($3.00 in Canada). Parts of volumes can be supplied at the rate for single numbers, $0.50 each. Missing numbers will be supplied free when lost in the mails if written notice is received by The Duke University Press within one month of date of issue. All remittances should be made payable to The Duke University Press.

Entered as second-class matter, August 18, 1932, at the postoffice at Durham, N. C., under the act of March 3, 1879.

COPYRIGHT, 1933, BY DUKE UNIVERSITY PRESS

CONTENTS OF LAST ISSUE

ERNEST SEEMAN, Durham, and ROBERT SAUDEK, London: "The Self-Expression of Identical Twins in Handwriting and Drawing."

MAJOR R. W. G. HINGSTON, London: "Instinct and Intelligence in Insects."

A. A. ROBACK, Cambridge (Mass.): "Writing Slips and Personality."

PAUL LAZARSFELD, Vienna: "An Unemployed Village."

WERNER WOLFF, Berlin: "The Tenth International Psychological Congress."

News and Notes: U. S. A., Germany, Aura. Book Reviews.

SOME FORTHCOMING ARTICLES

O. V. VERSCHUER, Berlin: "Inheritance of Mental Traits."

A. LEGRUEN, Vienna: "Twins Among Austrian School Children."

M. BLEULER, Liestal: "The Shaping of Personality by Environment and Heredity."

A. R. LURIA, Moscow: "Self-analysis and Social Conduct."

FRISCHEISEN-KÖHLER: "The Inheritance of Personal Rhythm and Tempo."

HILDEGARD HETZER, ANNELIESE BRAUN, Elbing: "The Development Test as Applied to Infants in the Service of Psychodiagnosis."

L. S. VYGOTSKI, Moscow: "Comparative Psychology of the Child."

ROBERT SAUDEK, London, ERNEST SEEMAN, Durham, N. C.: "A Study of Criminal Twins."

V. TILLE, Prague: "Personality in Literature and Life."

NOTE.—The Editors have invited a number of leading psychologists to discuss the question of the rôle and the limitations of experiment in psychological research. A generation ago the general attitude toward experiment was one of unbounded hope. Of late years opinions have diverged, and while some psychologists regard all efforts that are not strictly experimental and of the laboratory as outside the pale of Science, others incline to assign to experimenta a very modest scope.

This divergence of opinions was clearly manifested in the debates at the Tenth International Congress of Psychology. The discussion is opened in the present issue by Ernst Kretschmer, Hans Driesch, and William McDougall. It will be continued in following issues by Alfred Adler, Oswald Bumke, and Hans Prinzhorn.

THE EXPERIMENTAL METHOD TREATED AS AN INSTRUMENT OF PSYCHOLOGICAL INVESTIGATION

ERNST KRETSCHMER
University, Marburg

Experimental psychology was over-estimated by a non-psychological generation, only to be despised by the generation which followed. Neither estimate was deserved. At the present day the main charge brought against it is that it is sterile, loses itself in detail, and does not touch the core of the problems of the mind. Such a criticism, however, does not apply to experimental psychology as such, but to the manner in which it has frequently been applied; for experimental psychology is not a separate branch of psychology, but simply a technical instrument, and its sterility or fruitfulness depends entirely on the question to the solution of which it is applied. In psychology, as elsewhere, the experimental method is fit to cope with any problem and, more than that, it provides us with new and important problems.

All things considered, I believe that the experimental method has the same advantages and the same difficulties in psychology as in every other sphere.

Our first question is, whether the experimental method is the only one by which psychological facts can be ascertained. From time to time the assertion is made that the only scientific facts are those

which can be expressed numerically. Yet the whole history, e.g. of medicine, serves to refute this assertion; its imposing structure down to its latest details is composed almost exclusively of observations and concepts based upon sense data and intuition; the part based upon numbers is microscopically small. A vast number of the most important discoveries, discoveries which are convincing to every trained observer, are reached without the use of experiment or numbers, the only method being that of the scientific description of fine acoustic, optical, and tactile distinctions of breath sounds, character of the pulse, ophthalmoscopic examination, and microscopic shades of form and colour down to the most complicated disturbances and motion forms.

From this region we pass in the science of psychiatry and neurology without transition to the sphere of pure psychological observation. If a patient enters the room, the doctor is immediately in a position to diagnose him as a case of "shaking palsy," and this diagnosis embraces at a single glance and with a single term a vast body of typical means of behaviour both in the motor and in the psychic sphere. Furthermore this diagnosis furnishes us with a clear diagram of causes, prognosis, and possible treatment. It is easy to imagine how difficult and at the same time how incomplete would be the result if it were attempted to represent this empirical phenomenon of "shaking palsy" by seeking out its components through methods of experiment and number; while even if this method were pursued, it would be impossible to excel the compelling universality which the clinical view of "shaking palsy" possesses. This instance clearly shows the vast superiority of the method of observation and description over the merely numerical method.

The same might be said of the general symptoms of mania—a cheerful form of excitement where no single idea is held fast. Here again a clear and universally valid picture can be derived from the sequences of ideas and the forms of motion and can be pursued in the face of a definite formula in its clinical, hereditary, and characterological connections. The experimental method also has been applied to mania and the general view has been completed and corrected in detail; but the fundamental clinical view has not been altered.

The principles which we have just applied to certain psychopathic instances can also be applied to normal psychology. A mass of knowledge can be derived from direct observation applied by outside

observers as well as by self-observation; and the results of the latter can in turn be used in order to afford a sympathetic insight into the inner life of others, to say nothing of the vast psychological material latent in historical documents, biographies, and works of poetry. If all these important sources and spheres of investigation were left on one side and recourse were had solely to the experimental method in dealing with psychological problems, this entire branch of science would be impoverished to an almost incalculable degree: the experimental method is an important but by no means the only way to approach psychological knowledge.

Is the experimental method in psychology reliable?

Together with my collaborators I have spent years in studying the problem of the connection between the physical and psychical manifestation of personality. We began by attempting to seize as completely as possible and to classify the types by descriptive methods. Next we studied the same facts by purely numerical methods, viz., by measurement and experiment, and we succeeded by these means in reaching a purely mathematical formulation of the connection between body and mind (index of bodily structure, psychological experimental number). We have also compared the results reached by clinical descriptions with those reached by mathematical methods, doing so purely from an interest in the question of method. It was found that the two separate results were in close agreement, differing only in a few details, whence it can be inferred that both methods are scientifically valid and reliable, complementing each other and furnishing an excellent check on each other's results.

Naturally this applies only when the observer is trained; for the untrained observer both methods are disastrous, although from different reasons. The method of clinical, descriptive diagnosis is dangerous because it requires a superior capacity for the sense-apprehension of slight distinctions, while the danger of the mathematical method is that it is blind in itself and furnishes an answer only to an observer trained in the discovery and interpretation of numbers.

While both methods are reliable, the nature of their reliability is different. The advantage of the descriptive method is that it approaches facts much more completely and in their natural, significant connection; its disadvantage is that sense-psychological and linguistic reasons prevent it from reaching more than a certain degree of accuracy in its definition of the facts. The mathematical-exper-

imental method suffers from a much greater dearth of facts and from the purely empirical standpoint it invariably amounts to a clumsy abstraction which at best isolates a small number of factors from a complex of factors and treats them as representative of an infinitely more complex object. Moreover the factors seized upon are not those which are the most important empirically but those to which the numerical method can best be applied. To this must be added that in experiment as with statistics, there remains the problem of evaluation—of interpreting the number obtained. The fact is that even the simplest experiment contains a plurality of factors which have first to be gathered in their entirety and must be evaluated according to their importance for the numerical results. The advantage of the mathematical method consists in the fact that it reaches a degree of precision preventing any refutation in those few cases which it can apprehend and approach. Further, psychological experiments if well carried through almost invariably provide a number of useful observations apart from their immediate numerical results, these observations being made on the subject of the experiment apart from the immediate questions addressed to him. I myself have found such subsidiary observations extremely useful.

The essential parts of the method can best be explained by taking examples from biology, where these things can be seen much more simply and clearly. If I wish to study a certain type of bodily structure by mathematical means (i.e. by measurement), my chief materials consist in the dimensions of the skeleton and the circumference measurements. But these measurements, far from exhausting the biological facts, are no more than a small part; the type of hair or the skin colour often supply a more significant clue to the inner constitution of the body than the skeleton. Nevertheless, when applying mathematical diagnosis we begin by making use of the skeleton. Thus we see that the preference shown for a certain group of phenomena when the mathematical method is employed is not due to the fact that this group has any inherent value as interpreting the problem in question, but that it is better adapted to the numerical method, in other words, that it is more susceptible to measurement. Exactly the same applies to experimental psychology. The purely mathematical method invariably supplies a picture of a total empirical phenomenon relatively poor in observation and distorted from technical epistemological reasons.

Thus experiment can never be a substitute for descriptive obser-

vation. But these two methods complement and check each other and should therefore, if possible, always be employed together. If this is done the mathematical-experimental method provides us with a succinct and firm outline while descriptive observation furnishes the full wealth of empirical appearance in significant connections, by which means alone a correct interpretation of numbers is possible.

We must now ask whether experimental psychology has any theoretical limits.

We have seen that every kind of mathematical investigation has numerous contingent limits due to the degree in which it is technically possible to seize an event by measurement and number and to the artificial conditions of any experiment. To this extent there are numerous events and regions in the life of the soul which are technically unsuited for experimental and also for statistical methods. But the attempt has also been made to discover theoretical limits for experimental psychology on theoretical epistemological grounds and to establish a distinction between the sphere of introspective and sympathetic interpretation from that of the psychical events observed from without. Naturally a psychical experience cannot be measured directly; but this does not often happen in physics either; thus heat is not measured directly, but is inferred from a mechanical concomitant phenomenon, viz., the change in volume taking place in a column of mercury. The sublimest soul-events may be accompanied by contingent manifestations of a motor or vegetative kind which allow some part of them to be seized experimentally or by measurement. I have here in mind the profound emotional processes—processes of the finest psychical sensibility—which can be manifested by means of the psycho-galvanic experiment or by means of pulse and respiration-curves. These methods reveal not only the course but also the content of such experiences. Again there is a simple experiment by which the distinction between the sense illusions due to schizophreny and delirium tremens can be distinguished, the difference between the imaginary and symbolic character of the former being differentiated from the more sensuous character of the latter. The methods of experimental representation of the more subtle soul events are by no means exhausted.

Summing up, we may say that the advantages and disadvantages of the experimental and mathematical method are approximately the same in psychology as in other branches of science. With regard

to the number of facts apprehended and with regard to their detailed qualitative evaluation, the method of experiment can never rival descriptive observation. The latter method is employed in psychology by external observation but also in the form of introspective and sympathetic interpretation. On the other hand the method of experiment and mathematics furnishes a more definite proof of the facts which it can apprehend. It provides an excellent check and completion of other psychological methods and is productive of keener observation. It is the best preventative of vagueness and subjectivism.

THE EXPERIMENT: IS IT THE ONLY, AND IS IT A RELIABLE WAY TO ESTABLISH PSYCHOLOGICAL FACTS?

HANS DRIESCH
University, Leipzig

1. Before we can decide whether the experimental method is the *only* one available to establish psychological facts, and, further, whether it is a *reliable* method leading in that direction, it is necessary to lay down a clear definition of psychology and to ask what is likely to be the correct way to study psychological facts in view of their peculiar nature.

At all times the word "psychology" has been used to denote the facts of mental experience. This much can be regarded as certain. But it is equally certain that, in the first instance, the words "mental experience" are equivalent to conscious experience and this despite the fact that at a later stage theorists may speak of so-called unconscious dynamic factors which do not belong to conscious experience and are introduced simply to facilitate theoretical understanding. Accordingly psychology is the theory of conscious experience and of the laws which govern its chronological course.

A number of widely divergent views have been held with regard to the various approaches leading up to the object of the study of psychology.

The first of these views starts from the assumption that every kind of conscious experience is tied to some kind of ego and that consequently the only experience and the only ego which I know immediately is my own ego and my own experience. And the only way in which I can know it is by introspection, whence it follows that any genuine or pure psychology must be based exclusively on introspection or, at any rate, that introspection is the final court of appeal to which psychological knowledge has regress. It is introspection alone that provides me with experience and this in turn is the material of future investigation. And further, introspection

alone when added to recollection will provide me with knowledge of the chronological sequence of my experiences. The laws which govern these latter have to be investigated later from the standpoint of causality.

Even the introspective psychologist will frequently have to mention an external mentality; he will have to say that he experiences a "you," and in everyday life he is continually doing so; but at the same time he is aware that his acquaintance with this external experience can never be immediate. The reason is that any other person is given immediately merely as a corporeal manifestation in the shape of a "material system in motion"; and an argument by analogy is requisite (generally performed with extreme vagueness) before the movements of this material system (which includes its speaking and writing) are interpreted, as, for example, when I say: "The facts are of such a kind that these movements like those of 'my body' appear the results of conscious experience." Thus the introspective psychologist is fully aware that the mental life of any other person or animal can be studied only by the method which the American psychologists call "Behaviourism." The introspective psychologist radically adopts behaviourism as a *method* as soon as he has to deal with an external mind. At the same time he rejects the mechanistic dogmatism adopted by so many behaviourists, for it is far from being a priori certain that man regarded as an acting entity is a mechanical automaton; and proof is required before he will admit that a material system in motion is identical with mechanical automata. Indeed, the majority of psychologists, at least in Europe, holds the view that the material system known as "man" is not an automaton and that consequently the theory of psycho-mechanical parallelism which dominated psychology for such a long time was wholly erroneous.

The second view as to the means by which we can approach the object of the study of psychology is that adopted only by the younger members of the so-called phenomenological school in Germany. These are the disciples of Scheler. For Scheler, too, the prime method of psychology consists in introspection, but his theory differs from that just described in the view it takes of external minds. According to Scheler and his school we are supposed to reach these immediately and not by means of an argument by analogy which in practice is of a very vague nature. It is Scheler's doctrine that we are immediately to "perceive" the foreign mind or the "you," in the

same way in which, in another sphere, we perceive the "thing." According to him the idea of the "you" is given to us *a priori.*

We saw that Scheler and his followers employed introspection in the sense of immediate intuition as the sole method of psychological investigation, with regard both to their personal experience and to that of others. The third psychological school, on the other hand, which I propose to describe as that of *Radical Behaviourism,* absolutely rejects the method of introspection. As far as the study of external minds went, we found that the first way we described adopted behaviourism as a method although it did not go so far as to adopt mechanistic dogmatism; and we describe the view just mentioned as radical behaviourism because it goes further and rejects introspection even for the study of a personal ego: introspection is rejected in toto. Psychology comes simply to mean a theory of behaviour and of attitudes in animals and men; and it is assumed a priori that this behaviour and these attitudes which manifest themselves in material movements can be explained clearly and exhaustively in terms of physics and chemistry taken in their brief and most general sense—in other words, in terms of mechanics.

2. Personally I adopt without reservation the first of the three methods of psychological investigation described above. In other words, I accept ***introspection*** for the only purely psychological method, i.e., for the study of my experiences and their sequence; and I accept behaviourism interpreted by analogy for the study of other minds than my own.

Scheler's alleged immediate perception of external minds seems to me to belong to a group of errors which are current, e.g., with regard to the concept of the "thing," with the phenomenologists. These thinkers forget their own past history; they forget the fact, and the manner in which they did so, that they acquired, in early youth, concepts like that of "you," "thing," "cause," etc. When they assert that these concepts are given a priori, such remarks are meaningless because, as Locke knew, every concept exists potentially in the sense that it may be acquired and when acquired can later be employed as though it had been innate. The number of primary concepts, like "this," "such," "not," "so much," "whole," "related," "because," etc., which in fact are innate in man regarded as a rational being is exceedingly small. These are the elements out of which all the rest is formed by the help of empirical thought.

With regard to what we call radical behaviourism, it would

seem to us to be an utterly impossible theory, quite apart from the mechanistic dogmatism which is its foundation. Scheler took far too many facts as being given a priori, and was altogether uncritical in extending the a priori basis of the kind of psychology which he pursued. As for radical behaviourism, this theory deprives psychology of the only basis which allows it to exist as such, namely, introspection. At bottom radical behaviourism abolishes psychology proper as a science. For it, any theory of experience, of the Cartesian *Cogitare,* ceases to exist, and all that it will admit is a physiology of movement mechanistically interpreted.

We are justified in asking at this point what is the essence of those who insist on treating radical behaviourism as a scientific system; of those who make it the result of their thought. Is the process of their thinking also "matter in motion?"

We have here a new form of the crudest error of the old radical materialism; the radical behaviourist forgets that he himself is a mental being devising behaviourism by thought, and as such, experiencing.

3. We are now in a position to reply to the *first* questions forming the title of the present paper: "Is the experiment the *only* and is it a reliable way to establish psychological facts?"

After our enquiry into the nature and function of psychology we have no hesitation in replying in the negative.

I propose to define as an experiment every process by means of which a student deliberately interferes with an event under investigation in order to discover the result of this interference as far as the event is concerned.

We saw that introspection was the ultimate and indispensable foundation of psychology, at least in so far as it has to investigate the material under consideration. We saw that the rejection of introspection was tantamount to making psychology impossible as a science of experience. Now it is certain that introspection is not an experiment but simply self-examination. When introspection takes place I consider the totality of my experience; I classify it roughly, e.g., into matter of sensation, feelings, thoughts, volitions, etc., and then proceed to look for something yet more elementary, viz., not admitting of further analysis, which is to be found in every experience but each time in different combinations. Pure qualities, pure nuances of feelings, etc., abstract entities, criteria of truth and so

forth are in this way found to be elementary, as has been explained in detail in my work *The Crisis in Psychology* (Princeton, 1928).

Accordingly, so far from being the only method by which we can obtain material, the experimental method is altogether valueless for this purpose. It might be said, of course, that I might adopt the simple realistic standpoint and ask another person what are the elementary entities in his experience; and I might even indulge in a primitive kind of experiment, e.g., by placing numbers or pieces of paper of different colour before another person and asking him to enumerate the elementary parts of this experience: in this manner I should be able to discover colour-blindness. Nevertheless my introspection alone, directed upon my personal experience, will tell me what colour or sound is in the sense of something which is experienced.

It therefore is fairly certain that the experimental method is not the only one which stands in the service of mental dynamics, i.e., which serves to provide us with material, although we shall find that in this regard it stands by the side of introspection. Here, too, introspection forms the general basis in so far as we are dealing with mental events, i.e., the variety of experience as coördinated to time.

The fact is that here too introspection can provide me with the raw material which is worked upon later, the only difference being that in the present case we are not dealing with the raw material for every kind of theoretical work, but only wtih the chronological sequence of concrete experiences proper. For it is hard to see how this chronological sequence which, in its turn, forms the foundation of every study in causality—i.e., the transition from *post* to *propter*, or from sequence to consequence—can be effected without introspection, an introspection which in this instance has to be accompanied by recollection referring to definite dates. This is yet another instance where the radical behaviourist forgets a part of himself, namely, his memory. It is true that memory can be deceptive, and that, strictly, we can do no more than believe in the accuracy of memory in Hume's sense of *belief*. At the same time every science becomes impossible unless we hold this belief.

The mass of chronologically ordered experiences obtained by means of introspection combined with recollection has now to be studied causally. Here again introspection enters into force, and this it does in two forms, one of which might be called negative, and the other positive. In its negative form it tells me that I, as a con-

scious ego, do not "make" my experiences, and that on the contrary these come to me, to express it in popular parlance, as they will. In its positive form introspection and recollection inform me that the sequence of my experiences is frequently, and especially when I am reflecting on something, as it is called, of such a nature that each member of the series is richer in significance, truth, solution, or whatever it is to be called, than its predecessor. The negative as well as the positive result of intuition eventually leads up to psychogenetic theory proper. The former shows us that the actual causal dynamic factors lie in the unconscious, since, as we saw, the ego does not produce anything, while the latter informs me that the quasi-mechanical theory of association is theoretically inadequate even in connection with the concept of the "constellation." We find that causal factors of a directive nature tending towards an enrichment of sense or totality, have to take the place of merely associative factors, and that this has to take place within the sphere of the unconscious.

What has been described so far has been effected by introspection aided by recollection, and by these two alone without any experimental assistance. Any kind of experiment which, at a later stage, might assist introspection, is ultimately and inevitably based upon these results of introspection, since it is the case that all results of experimentation are interpreted through the results of introspection and recollection previously reached, and reached by these two alone.

4. We can now approach the question whether the experimental method is a valid one in some if not in all the spheres of psychological investigation by the side of introspection which will always remain its ultimate foundation, and whether in these spheres it is a "reliable" method of psychological investigation.

We said above that the meaning of experiment was to effect an arbitrary change in certain states of empirical reality, and to observe the results of such a change with respect to other states or with respect to events. "Arbitrary" can here be taken in its everyday meaning and there is no need to enter into the question of free will.

The only states which we can change are those which lie in the sphere of material being, *i.e.*, of nature, which includes the bodies of other persons or our own body—my "body" as it is for myself. Here the school which inculcates behaviourism as a method is entirely in the right.

The so-called "psychological experiment" too is always an ex-

periment within the framework of material nature, nor could it be otherwise, although it may well be the fact that material changes (including mere speaking as such) are used only as a means towards an end, viz., psychological discoveries. Such ultimate psychological discoveries are invariably the interpretation of material events proceeding by means of analogy on the basis of results reached by introspection.

Whenever a psychological experiment is undertaken upon another person, the assumption is made by analogy that his movements—and it is they alone which can be experienced immediately—are coördinated in some univocal manner to some process of the mind, the kind of coördination not being taken into special consideration. The question of this coördination is a special form of the general problem of body and soul.

There are a large number of divergent experiments all of which call themselves psychological.

The first group of these is not psychological at all, but purely physiological, and it is described as psychological only because its results are intended to be placed in some relation to psychological facts in the further course of an investigation. Among these is the discovery that the optic nerves cross each other, that the image on the retina is inverted, and that the brain contains certain points of motor excitation called centres. The second group of experiments usually described as psychological is in reality psycho-physical. It studies the relation between changes in the physical surroundings and the conscious experience supposed by analogy to exist in other persons. All experiments relating to Weber-Fechner's law, to periods of reaction, to the merging of colours on a spinning top, etc., belong to this group.

The immediate procedure in these experiments is on behaviouristic lines, as we must insist once again; i.e., what is immediately observed is the motion of other persons including their speech. Of course these motions have a meaning and are understood in this sense —in the sense of being another person's experience. But this would be impossible unless I knew by introspection the meaning of experience and the way in which my specific experiences are coördinated to the movements of my body. In practice the procedure here is entirely naïve and realistic. When the subject of the experiment says "I see white," "I am now perceiving," "This is a distinction," "I have now understood," etc., or when he manifests the contents of

these sentences by any agreed sign other than language, it is assumed that we are in an immediate relation to the mental life of the other person. This naïve realistic standpoint does no harm so long as we remember that it is a naïve realistic standpoint.

The third type of experiment is more truly psychological than the other two. But this type also inevitably proceeds on behaviouristic lines. It was first introduced into experimental psychology by the so-called thought psychologists and more particularly by Külpe.

In this type of experiment, to give a short example, a word denoting some kind of thing is called out and the person on whom the experiment is made is asked to pronounce a word denoting a coördinated or subordinate concept; he is further asked to state his "experiences" during this process. Or again some saying by Goethe and another one on the same subject by Nietzsche is pronounced and the subject of experiment is asked (a) to say whether he has understood the meaning of the two sayings and (b) to evaluate the two aphorisms, i.e., to say which he considers the more correct; finally, and this is the most important point, he is again asked to describe his experiences.

This type of experiment, which, like the others, is generally pursued, in practice in a spirit of naïve realism, is exceedingly remarkable. The first question we might ask would be *who* it is who is making the psychological experiment. Obviously the person conducting the experiment is the primary investigator. But the subject of the experiment is here far from being passive, nor is it the case here as it is in the second group of psychological experiments, e.g., where we are dealing with periods of reaction, that he is active only in so far as he utters some word which is taken as a symbol. In the present type of experiment the subject of the experiment is actively introspective and in fact it is he who makes the really important introspective investigation. It is the result of his introspection which is recorded and all that the primary conductor of the experiment does is to pass it on. 'It is true that he goes further and also compares this with other previously obtained results; but it also remains true that the experiment is really carried on by the subject of the experiment. This type of experiment has its drawbacks; e.g., the subject of the experiment during introspection runs a risk of discovering what he is expected to discover or wants to discover for theoretical reasons. But we need not enter on this point at the present moment.

In concluding the present exposition we may sum up the importance of psychological experiments and more especially of the thought-psychological experiment, by saying that it presupposes a naïve realism but that it provides useful aid by the side of genuine introspection, which is fundamentally solipsistic. At the same time and quite apart from the fact that it is ultimately based on introspection, i.e., the introspection of the subject of the experiment, it is not an indispensable method. The conductor of the experiment alone might discover all the essential facts which are discovered by himself, although we must admit that the results of the experiments might cause him to renew his introspection and to apprehend certain points originally overlooked and to see more clearly facts which originally had been but vaguely grasped.

5. All the above, however, applies only to the study of normal psychology. The case is altered as soon as we enter the sphere of abnormal psychology.

It is here and only here that the experimental method and with it the observation of another person becomes indispensable, although we must never forget that, when dealing with another person, we are invariably studying him on behaviouristic lines and that the psychical interpretation inevitably takes place merely by analogy.

Anything connected with hypotism, spontaneous or suggested illusion, hallucinations, negative hallucinations, post-hypnotic emotions due to suggestion, etc., can only be studied behaviouristically. Sometimes the objective behaviour of the hypnotized persons is simply observed and simply interpreted while at other times, but less frequently, the experiences taking place during hypnosis or after awakening from hypnosis are caused to be recounted, as in the thought-psychological experiments. But even here the knowledge of his own mind reached introspectively by the conductor of the experiment is the basis tacitly assumed to lie at the bottom of the whole process. Admittedly the law of the sequence of experiences is different in a hypnotized person from that of a normal person in an awakened state, and the conductor of the experiment cannot really understand the hypnotized person; but the materials of which the abnormal sequence is composed are the same as in a normal person; these are introspectively known to the conductor of the experiment and it is only because he knows them that he can describe the law of their sequence in the hypnotized person as "abnormal."

Everything belonging to the sphere of mania must be approached

on behaviouristic lines, though not by means of experiment. Every kind of illusion, e.g., megalomania or persecution mania, is a reality for the afflicted person, whatever its origin; and the observor infers either immediately from the words of the patient or indirectly by way of analogy from his general behaviour that the patient actually is afflicted in this way. He argues that if he himself were compelled to speak or act like the patient he would also be having these definite experiences, e.g., the illusion of being persecuted. Abnormality does not consist in the fact that the experiences are experienced, for after all it is possible to be persecuted in a normal way, but in the circumstances in which the experiences arise.

There is no need to emphasise that everything belonging to the sphere of "Psychical Research" must necessarily be studied on behaviouristic lines in so far as it is found in other persons and is observed or experimented upon in them; the person having the experience in question acts in a manner which, interpreted by analogy, compels us to reach the conclusion that he possesses a certain kind of knowledge. In the given circumstances he cannot have acquired this knowledge in a normal way; and it is in the *way* in which the knowledge is acquired that the paranormality of the event resides. And it is precisely here that the only possible manner of investigation is on behaviouristic and, if possible, experimental lines.

We can, then, sum up by saying that experiment and mere observation are a *reliable* method for the discovering of psychological facts and, moreover, that they are an important method, provided always that we remember that the experience of another person is inferred by analogy from his behaviour. This applies to abnormal and paranormal psychical events. But it does not follow that experiment and observation are the *only* methods of investigation.

6. This leads us to another and very important question; a question which, however, is not very frequently asked.

For every mental subject, i.e., from a strictly solipsistic point of view for "me," there are two distinct classes of human bodies, all of which, of course, fall within the group of organisms in general. One of these groups is represented by a unique example, viz., *my* body, while the second embraces the bodies of all other persons. We need not here discuss the question how *my* body is apprehended by *me* in a manner totally different from that in which all other bodies are apprehended. There is one question, however, relating to this distinction, which is of extreme importance.

We saw that alien bodies could be observed and experimented upon behaviouristically with regard to psychological problems which were reached by an argument of analogy. The question which now arises is whether I can observe and experiment upon *my own body* on behaviouristic lines. Or to put it in different words, can I establish a definite, univocal relation between states and events experimentally brought about, or simply observed, in my body, on the one hand, and my conscious experience introspectively apprehended on the other? If I am successful in this, the objectionable argument by analogy would be eliminated and it might prove possible to obtain literally extraordinary results.

It would not be difficult to devise this kind of *autoscopic* experiments, as we may call them for short, in the field of the psychology of perception. Such experiments might deal with perceptible differences; thus a sort of light of which the intensity is objectively determined, might be procured; the experimentor might then close his eyes and walk about feeling his way and might finally open his eyes and register differences of brightness where he perceives them. It is true that such and similar experiments would not be of any particular importance except methodologically.

A similar method might be pursued in thought psychology. Given the problem of finding a coördinate concept for a given concept, we might take a book and decide to find the coördinate concept, e.g., to the third substantive occurring on page 7, denoting a thing. We might then call to mind the experiences of this process; but here again it is not to be expected that any strikingly novel results would be reached.

Again, anyone who is a *metagnome,* to use the French expression, or, to use ordinary language, who possesses mediumistic powers, either in the way of spontaneous telepathy, or, to use Lehmann's term, by putting alien thoughts on tap, is of course likewise in a position to note his experiences for himself and is also in a position to call to mind critically the conditions under which his experiences took place; he is thus in a position to ensure himself against any illusion as to the paranormal character of his experience. Here again it is not so much a new experience that matters, as the autoscopic character of the investigation and the question of principle whether it is feasible; in other words, to establish whether the person in question may really be experimentor and experimentee in one.

It is further possible, of course, to investigate the effect of

poisons on the mental life in oneself, and in fact, this has been done. The same applies to the effect of tiredness upon correctness in arithmetical operations.

Radical novelities in psychological investigation can be looked for only from experiments which sound very thrilling and which only a thorough-going fanatic would undertake in the service of science. I have in mind experiments consisting in operations on the brain where a person's *own* cerebrum is operated upon. For it is solely by means of such "autoscopic" experiments that the relation between the brain centres and the brain functions and their connection with conscious experience could be determined univocally. All operations carried out on other persons and all observations made after injuries or accidents are not really univocal for the real central question, because, with another person, we can never do more than determine and interpret by analogy what he *says;* and he may be in a position of not being able to *say* very much about the experience.

On the other hand it might be the case that the results of autoscopic experiments with operations on the brain might remain practically solipsistic because the experimentor might not be in a position to say anything about his experiences and might be compelled to lock up his experience within himself; and further, supposing that the function of his brain were restored, as can be done, as we know from other persons, the case might be that he had forgotten the essential points by the time he was in a position to make normal statements about his experience.

Thus autoscopic brain experiments are really only of "academic" importance, although in this respect their importance is considerable. And for this reason alone nobody could fairly be asked to make the sacrifice which such an experiment involves.

7. If we proceed to sum up the various points discussed in detail, in order to discover the answer to the two questions raised above, viz., whether the experimental method is a reliable and whether it is the only method to discover psychological facts, then we are led to the conclusion that the answer to the second question must be in the negative.

It is quite certain that the experimental method is not the only one possible in psychology, and we might go on to say that the experimental method never reaches genuine psychical facts and events immediately, and that even where it reaches them indirectly,

it does so only when its results are interpreted by analogy based on introspection and its results.

On the other hand introspection might be described as the only way to reach a genuine psychology; and, at any rate, it is possible to imagine a type of psychology based only on introspection. Indeed, this would be the purest and most genuine form of psychology. Strictly speaking I know only my own mental experience; I know the meaning of experience only in reference to myself, and I know the contents of experience immediately only through the contents of my own experience.

Thus, while introspective psychology without experiment is conceivable, an experimental system of psychology not based on introspection is an absolute impossibility. We saw above that the radical behaviourists, as we described them, forget their own existence when they insist that their psychology contains no element of introspection. They attempt to erect a science after first annihilating the object of this science. For after all, psychology is the science of conscious experience and its laws.

We said above that purely introspective psychology was the only genuine kind. At the same time it would be pedantic not to place experiment by its side as a reliable method, and this despite the argument of analogy on which it rests, and which inevitably makes it an indirect instrument of investigation.

Thus when dealing with normal psychology we can use experiments as correctives for introspection and the results of experiment can render introspection more accurate and responsible. They can also preserve us (from the realistic point of view) from rash generalization. Thus the existence of optic, acoustic, and kinesthetic types can be discovered only by experiment. It is true that such considerations do not belong to pure psychology but to psychophysics, that curious intermediate region of scientific investigation. Similarly characterology is a kind of classification and as such can only be studied experimentally.

We showed above in detail that abnormal and paranormal psychology requires experiments; what we described above as the autoscopic method is also, of course, a kind of experiment.

Accordingly we describe the experimental method as a reliable method but add that introspection must always be the basis of its psychological application, for every experiment as experiment is, strictly, merely a fact within the material world.

To conclude, we may briefly mention a line of psychological investigation which describes itself as "understanding," and has recently obtained much popularity in Germany. I do not consider this a science in the higher sense, but at best a preparation for a genuine science. It is based entirely upon the argument of analogy which we have so frequently mentioned, and fundamentally all that it says is, "I understand the other person in the sense that I can imagine myself in his place." This kind of psychology is employed by poets and historians; but a scientific psychologist can employ it only as a means of collecting material. In the scientific sense this method "understands" nothing and does not even try to do so. We may add that even historians might do well to go beyond this so-called psychology of understanding, and learn to utilize a scientific psychology, including in their explanations the results of the psychology of the unconscious and the sub-conscious.

EXPERIMENTAL PSYCHOLOGY AND PSYCHOLOGICAL EXPERIMENT

WILLIAM McDOUGALL
Duke University

When the editors of this journal first invited me to take part in this symposium, I, though highly appreciating the honour, declined it, thinking that the free utterance of my views might be offensive to some of my colleagues. But when the invitation was renewed, I accepted it. For in the interval I had read some reviews of my last published volume.[1] In that obscure booklet I had let myself go, throwing aside all discretion and all regard for decency; and my critics seemed to approve the change. They suggested that the mask of a gentleman, formerly worn, had cramped my style; and they agreed that my indecent little tract was the best thing I had written. In another recent publication I had thrown off another discreet mask, the mask of the severely impartial agnostic scientist and had figured as a defender of religion. And this effort also was widely approved.

I reflected, therefore, that discretion is, perhaps, not always timely; that perhaps I had reached the years of indiscretion, an age when it becomes one's privilege and, perhaps, even one's duty to speak one's mind without restraint and without scruples; that if, having set out with a scepticism more thorough than Descartes' and almost as complete as Hume's, one has been led by forty years of intensive toil to certain convictions, those convictions should be recorded in print.

This article, then, is an addendum to my *World Chaos,* continuing the theme and written in the same reckless spirit. My thesis was that the chaotic state of the civilized world is due to Science and that only by the aid of more Science can we hope to set it right, to make chaos give place to order, security and freedom. As thus stated, the thesis seems paradoxical. But on fuller statement the thesis loses the appearance of paradox. The remedy proposed is not homeopathic. For the Science which is to cure is not the Science which has been the cause. The cause of our chaos has been the unfortunate and very great predominance throughout the modern period

[1] *World Chaos, the Responsibility of Science.* London and New York, 1931.

of the physical sciences, together with the backwardness of the sciences of life and, especially, of the sciences of man and of society. The only possible remedy is the advancement of the sciences of the last group to the point where they shall afford some sure guidance in political, economic, and social problems of every kind.[2]

In 1869 John Stuart Mill, one of the great pioneers of the social sciences in general and of economics in particular, wrote: "Her [Britain's] thinkers are again beginning to see, what they had only temporarily forgotten, that a true Psychology is the indispensable scientific basis of Morals, of Politics, of the science and art of Education."[3]

It is notorious that the political economy of Mill and his school has been found hopelessly inadequate as a guide to action. And this failure of a system of economics avowedly based on psychology has been a main cause of the continuing neglect and repudiation of psychology by the economists and other social scientists.

But the failure has not shown that Mill, in claiming psychology as the indispensable basis of the social sciences, was in error. The failure was due to the inadequacies and positive errors of the psychology applied by him, a strange mixture of mechanical associationism and of hedonism, two false systems that cannot be blended into a whole, no matter how intimately they be mixed.

Shortly after Mill wrote the words cited above, the redemption of psychology by the introduction of experimental methods was widely heralded and high hopes were raised that psychology, now to be made truly scientific, would soon provide the required foundation for the social sciences. Yet, after full fifty years of "experimental psychology," the social scientists for the most part either continue to ignore psychology or turn aside from the "strictly scien-

[2] On the outbreak of the recent pyrotechnic display by the Technocrats, I was told that the thesis of my *World Chaos* was identical with theirs. This implies a complete misunderstanding. My proposal is the opposite of theirs. While they seek to exalt still more the already excessive influence and prestige of the physical scientists and the engineers, I urge that, in theory and in practice, these should be relegated to their true place in the life of society, one of subordination and subservience. They propose to push to an extreme the chief error of our western civilization, the source of all our woe, namely, the neglect of man in favor of mechanism; while I urge that we may hope to secure the future of our civilization only by taking seriously the old adage, "The proper study of mankind is man."

[3] A few years later (1874) Franz Brentano proclaimed psychology to be *the* science of the future and, no less confidently than Mill, looked forward to seeing it accepted as the basis of the social sciences (in the first chapter of his *Psychologie*).

tific psychology" of the laboratories to construct a *geisteswissenschaftliche Psychologie* which shall meet their crying needs.

How explain this failure of the experimental method to bring us, during fifty years, appreciably nearer to a socially useful psychology? The workers have been many and enthusiastic; the published researches form an imposing mass of printed matter. Were the hopes of the experimental method ill-founded? How comes it that, in the year 1933, we stage a symposium on the value of experiment in psychology? It is impossible to imagine a parallel debate among the physical scientists.

Wilhelm Wundt, who is justly regarded as the founder of "experimental psychology," wrote at an advanced stage of his career a social psychology (*Völkerpsychologie*) which bears little or no trace of his long labours in the laboratory and little relation to his experimental methods. An ominous fact surely for anyone whose critical powers were not paralyzed at that date by Wundt's immense prestige!

I well remember how earnestly, as a young man trained in the biological and medical sciences, I toiled through Wundt's massive tomes. Especially his *Outline of Psychology,* his most systematically imposing treatise, I read with all humility, believing that I had but to master it in order to have understanding of the human heart and intellect, and blaming upon my own stupidity the fact that every page baffled and perplexed me. For my faith in the experimental method was undimmed; and here was the ripe wisdom of the master of that method applied to the human mind.

Now, after more than thirty years of continued effort in the same field, I read again the same volume with cynical amusement. Now it seems to me a quagmire of pedantry, a mass of confusion and error, lacking even the modest merit of internal consistency. And I seem to see not only the errors, but also how and why they were made. At this date any reasoned justification of this estimate would be tedious and otiose. Wundt's views are no longer discussed; a fact which is, perhaps, evidence that progress has been made.

Among the host of Wundt's ardent pupils two were his outstanding disciples, Külpe and Titchener. Their later courses were significant. Külpe, an original and powerful intellect, broke away and struck out a new and radically different line of experiment. Titchener remained to the end the faithful follower and successor of the man whom he regarded as his great master. He refined the

Wundtian "experimental psychology" by pruning away some of its major inconsistencies; he purified it until it became what Wundt (in some passages) had proclaimed it should be, the description of "immediate experience" through introspection under the controlled conditions of the laboratory. Titchener added a new condition, namely, that the observer must be well trained; he showed how, by exerting professorial influence, in the good old German fashion, it was possible to raise up a group of students as docile as a troupe of poodles jumping through hoops, and, by their aid, to obtain wonderful results. The crowning achievement of his career, and of this ultra-Wundtian refinement, was the conclusion that pleasure is bright pressure while pain is dull pressure. With the announcement of this momentous conclusion, the method seemed to have reached its ultimate goal, to have completed its task, to have solved the last of its problems, the problems visible and accessible along this strait path. It was at once the crowning achievement and the *reductio ad absurdum* of the Wundtian principles.[4] "Experimental Psychology" of the strict Wundtian type may be said to have died of pernicious anaemia under the too drastic purgation treatment of Dr. Titchener.

But experiment in psychology did not necessarily stand or fall with the Wundtian principles. It goes merrily on. Clearly, "psychological experiment" is a term of various meanings; *tot sententiae, tot homines.* May I illustrate with anecdote? When I was a young man I had the pleasure of dining one day at the home of Sir David Ferrier. It is probably necessary to inform my younger readers that Ferrier achieved fame as one of the first to map out the areas of the brain-cortex by means of surgical experiment on the living animal. He was also a successful and fashionable West End London surgeon, in the hey-day of West End surgery. When we had progressed through sherry, claret, and champagne to port, my charming host became confidential. He surprised me with the information

[4] Titchener's esoteric principles could thrive only in a cloistered environment. Such an environment he found on the Cornell Campus where he enthroned himself. He was not one who "drank delight of battle with his peers." He did not attend the gatherings of the American Psychological Association or of the International Congress. He seldom left his campus. Once a year he gathered together a hand-picked group of such psychologists as were willing to follow, or seem to follow, him. When I first came to America I was invited to attend these gatherings. I think it was at my second of such reunions of choice spirits that I ventured to suggest that an experiment reported by Titchener might with advantage be repeated with a group of children as observers. I had known that I was regarded with a doubtful eye, and that my suggestion was temerarious; I was, therefore, not surprised when it appeared that my profanity had cost me the privilege of membership in this highly select group.

that at one time he had studied under Wundt; and then he surprised me still more by asking me—Did I know that Wundt had never made with his own hands a psychological experiment? "Surprise" hardly describes my condition. I was flabbergasted. Concealing my emotion as best I could, I continued to listen; and it soon appeared that, for Ferrier, psychological experiment meant nothing more nor less than a surgical operation on the living brain; and thus his surprising assertion assumed the guise of literal truth.

The incident was a critical one for me. As I sipped my host's excellent port and contemplated his white waistcoat, his suave and portly butler, his table covered with fine glass and costly wines, I inclined to think Sir David had chosen the better part, that I should do well to follow him along the primrose path of the West End consultant which leads to wealth, titles, and honours. But, in the cold light of the following day, it seemed to me that Ferrier's remark showed a Philistine lack of comprehension of Wundt's aims. Better, I said, a dinner of herbs than all the fleshpots of the West End, if only I may continue the struggle to comprehend Wundt and the secrets of the human mind. "I will not cease from mental strife, nor shall the sword fall from my hand, till we have built Jerusalem in Britain's green and pleasant land." For it seemed obvious to me then, and it seems still more obvious to me now after thirty more years of labour in the field, that, if we are to "build Jerusalem," the first and most essential step must be to acquire a more adequate understanding of human nature; that the social sciences we so urgently need cannot be built on the crude *ad hoc* assumptions about man made by a Rousseau, a Bentham, or a Comte, nor yet upon casual theories of human nature thrown off in leisure moments by historians (James Mill), economists (Adam Smith), logicians (J. S. Mill), engineers turned philosopher (H. Spencer), art critics (G. H. Lewes), or physicians of speculative bent and ready pen (Mandsley, Mercier *et hoc genus omne*); that the psychology on which alone the social sciences can be reared must be the culmination of all the converging natural sciences; that it must be the most complex, the most difficult and the most concrete of the natural sciences; that it can be brought to the point of development at which it may play its indispensable rôle only by the life-long labours of many men, men who have prepared themselves in the most elaborate fashion and who will bring to bear on their task every resource of modern science, especially the experimental methods.

Does this conviction seem at odds with my statement that "experimental psychology" has died a natural and not undeserved death? To remove the seeming inconsistency I must insist on a distinction of some importance. Reversion to my anecdotal refrain may make clear this distinction and at the same time show why I have been much concerned with the topic of this symposium and how I have arrived at my present position.

At the time of my visit to Ferrier, I stood at a parting of the ways. I had elected to devote myself to "the new science of experimental psychology"; but it was still possible to retreat. After my return from the Cambridge Anthropological Expedition to Torres Straits and Borneo, I had been invited by James Sully, (then Grote professor of Mental Philosophy in the University of London) to teach "experimental psychology" in his department; and, after spending a year with Professor G. E. Müller at Göttingen, had been advanced to the position of Reader in that field. I should explain that a Reader in an English university is half-a-professor; he receives a small stipend and is expected to give about half his time to the duties of his office. Thus I was free to become, a few years later, also Reader in Mental Philosophy in the University of Oxford. At the time mentioned I was thus two halves of a professor. One half taught and practised "experimental psychology" in London; the other half taught mental philosophy in Oxford and was strictly forbidden to dabble in "experimental psychology."

The question—What is experimental psychology?—was, then, for me not only of theoretical interest, but also of some practical importance. For the founder of the Oxford Readership had inserted in the governing statute a clause expressly forbidding "experimental psychology." He was a man of no wide culture whom a happy knack in winding armatures (I believe) had led to a large fortune and to fellowship in the Royal Society. He had also acquired an unbounded admiration for John Locke; and he desired to devote a little of his well-gotten wealth to the propagation of the doctrines of Locke. Hence the Readership. And Locke had made no experiments; therefore his Reader must make none. Before my appointment to this readership I had published a number of experimental studies; so my pious founder, who, unlike most pious founders, had the bad taste to be alive, kicked against my appointment and sought to dislodge me. When I published my first book (a primer of physiological psychology) he turned up at my lecture one day with

a copy of my book in his pocket: for the book, though it described no experiments of my own, was at least "strong presumptive evidence of a misspent youth." Fortunately, Oxford, though it did not believe in psychology, did believe in academic freedom; and when, after a short period of dual allegiance to London and Oxford, I was able to find a very capable successor to my London post (Charles Spearman) and transferred my experimental half to Oxford, that half was permitted to disport itself in a set of rooms in a corner of the physiological laboratory kindly lent me by the head of that department. I even ventured to lecture on hypnosis with practical demonstrations, a course which drew a large audience of undergraduates and dons: for such experiments required no brass instruments: and, for practical purposes, the line between "experimental psychology" and mental philosophy, so difficult to draw in theory, might well be defined by the brass-instrument-test. About the same date the University offered an honorary doctorate to Wilhelm Wundt; and that act seemed to me a pretty good guarantee that it would continue to turn a blind eye upon my experimental delinquencies.

But I was becoming more critical of Wundt. Dr. C. W. Saleeby, a high-class medical journalist, had written in a leading journal a review of two books, an edition of Wundt's *Physiological Psychology* and G. F. Stout's *Groundwork of Psychology*. He contrasted the two books, most unfavourably to Stout's; and he asked why British science could produce only such a puny and distant imitation of this great German masterpiece. Though agreeing with Saleeby in deploring the gross neglect of psychology by British universities, I was stirred to defend Stout. Stout's earlier and greater work, the *Analytical Psychology,* had been, I remembered, cruelly lampooned by another leading journal. I wrote an indignant reply to Saleeby's belittling remarks on Stout's work, in the course of which I said that Stout was to Wundt as a fine mechanician to a brick-layer. Of course, the leading journal refused to publish my reply. But I sent it to Saleeby; and, though he did not admit my impeachment, he took my strictures in good part; and when, a few years later, I published my *Social Psychology,* he was the only reviewer whose estimation of it agreed pretty nearly with my own.

At this time Sigmund Freud was rising above my horizon as a new star, scintillating but of uncertain magnitude. Was this the true line of advance for psychology? Were Wundt and his brass

instruments but a false and fleeting flare? The question—What is experimental psychology?—was becoming more than ever difficult. Was dream-analysis an experimental procedure? At the same time "mental testing" was beginning to make a noise in the world; and with the aid of several pupils (Cyril Burt, J. C. Flugel, and others) I was devising a set of tests designed to be universally applicable. Was "mental testing" psychological experiment? We were constructing and using brass instruments galore; our procedures stood up well to that test. But when I asked one who had made many productive psychological experiments—What is P. E.?[5]—he replied at once—"Of course, one would not include mental testing." That verdict would rule out not only about half the work done by American psychologists in the last twenty years, but also the work of Spearman and his colleagues, which in my opinion has made a contribution of high importance. Our tests were intended primarily to forward such comparative study of the native endowment of the varieties of the human species as we had begun in our work on the natives of Torres Strait in 1898. And in my opinion such work is an important part of the task of psychology.

It is clear that the answer to the question—What is P. E.?—depends upon the answer to the larger question—What is psychology? If, with Wundt, we define psychology as the direct study of "immediate experience," we rule out all experiment except introspective reporting under laboratory conditions; we rule out all experiments with animals (well nigh a second half of the American work of the last twenty years); we rule out all "mental testing" and almost all work on children; we rule out all observation of behavior under controlled conditions (including most of the elaborate work on memory of Ebbinghaus, Müller and others); and we have left only such work as Titchener's, culminating with the "discovery" that pleasure is bright pressure.

As early as 1904 in writing my first book, I had been much concerned to find a satisfactory definition of psychology. To mark off psychology from philosophy on the one hand and from other sciences concerned with living things has been one of the standing difficulties. The short-comings of Wundt's psychology seemed to me to be due in part to his mistaken definition of the science. I was certain that all such definitions of it as the science of consciousness, or of experience (immediate, individual, inner, conscious, or what not) were

[5] For the sake of brevity, I use throughout the rest of this article the symbol P. E. to stand for "psychological experiment."

gravely at fault, in spite of the elaborate displays of philosophical sophistication with which they were put forward, by Wundt, by Ward, and by many others. I suggested—Psychology is the positive science of conduct or behavior; and I renewed this suggestion in my next book (1908). I still hold that this is not only logically defensible, but perhaps the best brief definition. But, though it has been widely adopted, I now no longer adhere to it, for two reasons: first, it seems, or may be taken by careless readers, to ignore or belittle the rôle of introspection;[6] secondly, it fails to point to what is (as I have seen increasingly clearly with the passage of the years) the most central part of the task of psychology, namely, to achieve understanding of the organization which underlies and expresses itself in all the activities of men, not merely a static description of that organization, but an understanding of its development and functioning. What is Man? That is the great question which psychology must seek to answer. To define it less widely than as the science of human nature and its varieties is to narrow its scope and hamper its development.

The ordinary objection to so wide a definition is that there are other sciences of man, and psychology cannot claim to comprize them all within itself. My answer is that all the other sciences concerned with man are highly abstract. Each one deals with some part or aspect or function of man in abstraction from the whole of this most complex of all organisms;[7] anatomy and histology with his physical structure; physical anthropology with his anatomy comparatively viewed (while social anthropology is either psychology or a bare catalogue of uninterpreted facts); physiology and pathology with the functioning of his several tissues and organs. As soon as the physiologist considers the functioning of the whole organism he comes over into the field of psychology.[8]

What, then, of general biology, of genetics and morphology and embryology, etc.? My answer is that biology in all its branches is subsidiary to psychology, exists or is properly cultivated for the sake of psychology, each branch being pursued in abstraction from the main problem for the sake of which it is cultivated. Of course, there are practical bread-and-butter services rendered incidentally by the various branches of biology; corn and bacon are improved and two

[6] Closely connected with this is the practical reason that the behaviourists have taken up this definition and, in doing so, have spoilt it for general use.

[7] In this matter I am at one with the views of J. S. Haldane, best expressed in his recent *Materialism* (London, 1932).

[8] As so well illustrated in the concluding chapters of Sherrington's famous book.

blades of grass made to grow in place of one. But the one grand problem to which all the biological sciences contribute and, in doing so, justify their existence, is—What is man? Do I, then, include all biological studies within psychology? By no means. Then how do I draw the line? By reference to the purpose and effect with which observations are made, to their bearing, or lack of bearing, on the central problem, the nature of man. A few illustrations will best make clear the principle of division. If one applies an intelligence test to the children of a school simply for the practical purpose of sorting them into classes, that is not P. E., no matter how good the test or how much elaborate apparatus it involves. But if, as by Spearman, the simplest test is applied with the purpose, more or less successful, of throwing light on the nature of man, that is P. E.

If one takes alcohol in order to drown one's sorrows, that is not psychology; but if one takes it, or administers it, with the purpose and effect of throwing light upon the organism and its functioning, that is P. E.

If one keeps a monkey in the house as a pet and teaches it a few tricks, that is not psychology; but if one makes an infant chimpanzee a member of one's household and brings it up alongside a human infant of the same age under conditions as nearly as possible the same, and systematically compares the behavior of the two creatures, that is P. E.

If one observes the behavior of a bee with a view to avoiding her sting or stealing her honey, that is not psychology; but if, with the purpose of throwing light upon instinctive capacity and its relation to intelligent action, one scratches successive holes in the wall of the cell which a mason-wasp is building, that is P. E. And similarly of all study of animal behavior: even when (as in the classical work of Thorndike on cats) the observer's purpose is to show that nowhere in nature is there any purposive activity, his work is psychological in so far as it bears effectively upon the problem.

The principle of delimitation here defined is, in fact, the one which has become increasingly recognized in practice. It is time that we recognize and accept it explicitly. When psychology is thus broadly defined, there can be no further dispute as to whether experiment has a place in and value for psychology. We shall agree that the observations which provide all the raw data of psychology should be made as far as possible under controlled conditions, i.e., should be experimental. The only question at issue can be—How and how far

can experimental methods be pushed into new fields, e.g., into social psychology? The study of "immediate experience" or of "psychical events" (whether under controlled or uncontrolled conditions and freed from the atomistic pre-suppositions which have so hampered it in the past) may still bring refinement of description and may continue to flourish as a special department or as a propaedeutic to other psychological work, whether under the banner of phenomenology or that of *Gestalt* psychology. But never again can it be regarded as the whole of psychology,[9] or allowed to infect psychology with crypto-solipsism, as it did the Wundtian doctrines.[10]

Taking this generous common-sense view of psychology and of the field of P. E., we may ask—What has P. E. accomplished? During the fifty years of P. E. our knowledge of man has undoubtedly made some progress. One has only to re-read the books of the Victorian era in order to be convinced of this. But in what degree has this progress been due to P. E.? In my mature judgment, P. E. (in the most generous meaning of the term) has been only a minor factor in this progress. What, then, have been the main factors? Two such factors have been of outstanding importance, greatly overshadowing P. E. First, the general advance of evolutionary biology, with the comparative study of *homo sapiens* as one among the many species of living things. Secondly, the clinical studies of nervous and mental disorders by Broca, von Monakow, Flechzig, Meynert, Hughlings Jackson, Charcot, Janet, Marie, Head, Freud, Bleuler and many others (supplemented, of course, by such experimental studies as those of Goltz, Hitzig, Head, and Franz).

Adhering to my newly adopted principle of indecency, I illustrate my thesis by reference to my own work. In 1908 my *Social Psychology* was published. I seem to see now, far more confidently than at that date and in the light of a very much fuller knowledge of the history and present state of science, that that semi-popular little book made a considerable step of advance. It gave more definite form

[9] I would remark in passing that the leaders of the *Gestalt* school seem to put forward their great principle as a panacea for all the ills of psychology and the solution of all its problems; and in so doing they make the mistake of seeming to claim that the description of psychical events according to their improved principle may be the whole, or the essence, of psychology, thus repeating in a new form the fundamental error involved in Wundt's definition of psychology. Of course, in so far as their treatment is dynamic, it transcends the implied limitations, as we see most clearly in the dynamic development of Dr. Kurt Lewin's work.

[10] The psychologist, like all other scientists, must get rid of solipsism by an act of will before he takes up scientific work; on pain of lapsing into absurdities such as the doctrine of Mach and Pearson that the world consists of sensations.

and substance to the teleological, the hormic, the Dionysian view of man, the view adumbrated by Aristotle, by Schopenhauer, by von Hartmann, and by Nietzsche; it formulated for the first time an adequate theory of action, of conduct and of character; it provided a first sketch of the much needed psychological foundation for ethics and all the social sciences; a foundation which, though it will be improved in detail, will, I feel sure, stand good in its general principles.

My training in P. E., under Rivers and Müller, and my many experimental studies of problems of sense-perception contributed little or nothing to this achievement. It was made possible by training as a medical biologist, by wide studies in comparative and evolutionary biology, in physical and social anthropology, in natural history, in physiology, pathology, and all the medical sciences, and by the habit of spending much time in the society of my children.[11]

Why, then, has P. E. been so relatively sterile? Why has it contributed so little to the unmistakable advance of the last fifty years? Why did not the introduction of the experimental method lead to a rapid and victorious advance along all the front; as it did in the physical sciences? Why, in spite of much P. E., does psychology remain so chaotic that it is still looked at askance by both philosophers and the physical scientists, and ignored by the workers in the so-called social sciences?

One answer, popular among physical scientists and the public, is that psychologists are poor fools who allow themselves to be befogged by metaphysics. This answer was, for example, implicit in a recent utterance of a distinguished physicist. Professor R. A. Millikan is reported as asking—To what do we owe the brilliant advance of the physical sciences?—and as replying that we owe it to the wisdom of the physicists in confining their efforts to problems that can be solved by experiment. Now I do not deny that the physical sciences have had more than their fair share of first-rate intellects among their devotees. I deplore the fact and see in it a main cause of our present world chaos. But this answer contains only a small part of the truth.

[11] This early work of mine was lacking in respect of the insight to be gained along the other main line of advance, the study of the nervous and mental disorders: even as the work of Freud and of all his followers and derivatives is gravely defective by reason of their neglect to approach human problems by way of comparative and evolutionary biology, their ostentatious ignoring of biological science. It is up to them to remedy this defect, as I have sought to remedy the defect of my early effort by concentrating on nervous and mental disorders, endeavoring to bring together these two main lines of advance.

A principal ground of the relative sterility of P. E. hitherto is that psychological science is much more difficult than physical science. If any one questions this proposition, let him reflect on the attempts of men of acknowledged eminence in the physical and mathematical sciences to throw light on the human mind. Descartes was chiefly responsible for bringing "ideas" into the mind, for leading psychology along "the new way of ideas," which (with its representative or copy theory of cognition) has been a chief obstacle to the progress of psychology for nearly three centuries. And his treatise on the passions, with its inclusion of desire among the primary emotions, was full of confusion; while his pineal-gland speculation was a manifest absurdity. Leibnitz invented the too ingenious theory of pre-established harmony, which, I suppose, no man has been able to believe, but which nevertheless was the progenitor of psychophysical parallelism, another great bar to progress. And his doctrine of *petites perceptions* and their summation was another instance of excessive ingenuity putting psychology on a wrong scent. The great Newton himself was hopelessly lost in the toils of fruitless speculation when he pondered the problems of the soul. Ernst Mach pretended to solve the problems of space-and time-perception by the easy but manifestly absurd assumption of sensations of space and time. Helmholtz made important contributions to psychology, but he also made grave errors, as when, adopting Thomas Young's theory of colour vision, he ruined its prospects by attaching to it impossible theories of contrast and after-images. G. T. Fechner, another able physicist and undoubtedly a most original mind, spent half a lifetime on psychological experiments, the outcome of which was—as William James said—exactly nothing. And, to take a modern instance, Bertrand Russell, when he wrote a book on psychology, merely elaborated once more the errors of atomistic sensationism which had been a chief bane of psychology for at least a century.

The grounds of the relative difficulty of psychology and of P. E. as compared with other experimental sciences are such as we cannot hope wholly to overcome by any refinement of technique. For physical science it has, until very recent years, been a sufficiently good assumption that all atoms of one kind are strictly alike and any collection of such atoms a true sample of all. It is, therefore, possible to make the crucial and demonstrative experiment, the experiment which (if properly conducted) leads at once to an inductive generalization of general validity. Such a crucial and demon-

strative experiment, leading at once to a general truth, can never be made in psychology. P. E. necessarily deals with a total organism; and every organism, even amoeba, is vastly complex, a unique organization of many parts. No matter how precise our methods, we cannot safely assume that the observation made on one organism will hold good of another, not even in the case of a pair of identical twins. The experiment of the physicist or chemist deals with a vast sample of similar units, and a single observation has at least statistical validity. In P. E. statistical validity of a very inferior degree is attainable only by repetition of the observation on many individuals. The individual organism is to the psychologist what the atom is to the physicist; and the idiosyncrasies of organisms are, it would seem, much greater than those of atoms.

Not only is each organism unique by reason of its innate constitution; it is also unique in a second degree by reason of its history: for, in a sense, its history is incorporated in it in a way that finds no parallel in the inorganic world.

How baffling the problem of distinguishing the innate from the individually acquired peculiarities of any organism! This fundamental problem, on the answer to which so much else depends, has been urgently debated since the time of Plato; and still the most opposite answers are returned by competent students. It is a whole group of allied questions which we properly and inevitably ask. The claim to have found answers by observing what new born infants do and do not do is childishly absurd. What is the native basis of spatial perception? Are the psychoanalysts right in asserting that the horror of incest is innate? How much of truth is there in the legend of the birth trauma? What experiments can solve these problems? Even a large scale experiment in complete ectopic gestation (if it should ever become possible) might well fail to bring clear-cut answers.

And not only are experiments in this field intrinsically difficult in the highest degree, but also the experiments by the aid of which we might hope to obtain some light are for the most part morally objectionable. If we could observe a group of infants growing up in isolation from all personal influence, how much we could learn! Or again, if we could perform vivisectional experiments on the human subject, what a flood of light might be thrown on many questions of the deepest interest! But the moral difficulties are forever insuperable.

Contrast the experimental labours of the physicist. With some little skill in glass-blowing and in the control of radiation, he makes in a few days, an observation that recasts the constitution of the universe. Consider the procedures by aid of which Millikan and others have studied the cosmic ray. The instrument, the electroscope, a toy which any child could make and operate, is carried from one place to another; and the cosmic ray is revealed by a few readings of the utmost simplicity and becomes an established fact of nature.

Again I illustrate from my own work. The fundamentally important Lamarckian problem has long divided the biologists of the world into two groups, the deniers and the affirmers, and is deeply involved in a mass of emotional prejudices. The question is one of simple fact. Is the facility acquired by effort transmitted in any degree from parent to offspring? Nature seems to present to our contemplation millions of instances of such transmission. The question is of the profoundest theoretical and practical interest. Here, it would seem, is one question in which a decision may be reached by a crucial experiment. A single instance of such transmission established beyond doubt would transform the biological sciences, would vastly extend the field of psychology, would raise it from the position of an outcast hovering uncertainly on the fringes of the biological sciences to one of dominance over all the rest.

The brightest intellects, Charles Darwin, Herbert Spencer, Samuel Butler, Bernard Shaw, and many others have debated it pro and con. The whole lore of natural history has been ransacked for evidences. And in a multitude of cases the argument based on the evidence has been logically defective; so subtle and complex is the problem. Weissman, the most famous German biologist of his time, performs a ludicrously inadequate experiment on twenty generations of mice. Kammerer gives half a lifetime to experiment on the problem; is denounced as a liar and commits suicide. Pavlov, the most famous physiologist of our time, announces an experiment giving a clear-cut positive result: and, a few months later, revokes completely. My own arduous labours, extending through twelve years and involving the elaborate individual training of more than a thousand animals of thirty-three generations, seem to give clear-cut positive results. Yet so many are the possibilities of error and of alternative interpretation, that the experiment makes no impression on the hard hearts of the biologists.

Are we to conclude that this question is beyond our powers to

solve? Is it perhaps only one of many such questions, questions too difficult for the feeble powers of the human intellect? One could make a long list of problems that seem equally recalcitrant: the problem of the psycho-physical relations; the problem of the nature of subconscious activities; of the localization of functions in the brain; of the voluntary initiation and control of bodily movement; of alleged supernormal powers such as telepathy and clairvoyance; of the reality of imageless thought; of the place in nature of the secondary qualities; the problem of memory or, as it has been called, "mnemic causation"; all the problems of mental evolution.

In this connection "behaviourism" becomes a significant phenomenon. Suppose I make a resolute effort to accept its teaching. Perhaps, after all, if the crudities of reflexology are corrected by the principles of configuration (Gestalt), a synthesis of Pavlov and Köhler will begin to achieve satisfactory explanations of animal and human behaviour. But then I close my eyes; vivid pictures of scenes and incidents of fifty years ago pass before me, and all the old problems rise up again, refusing to be ignored. No! Behaviourism is an acknowledgment of defeat, an admission that the problems of psychology are too difficult for the human intellect. Yet, as I have argued in *World Chaos,* the future of our civilization depends on our success. If we cannot greatly advance our understanding of human nature and human society, we must all go down together; perhaps to make a fresh start when fifty centuries of stringent selection shall have produced a race of higher powers, one less inadequately equipped for the higher life.

A second main ground of the relative sterility of P. E. is one that is not intrinsic and may, therefore, be overcome. It is that experimental psychologists, aspiring to make their work truly scientific, have, for the most part, accepted physical science as their model; and, influenced unduly by the example of the physicists, have accepted as dogmas for their own guidance, strict determination, mechanistic causation, and the atomic principle, the principle of regarding the whole as the sum of a multitude of ultimate entities. Especially they have identified (some explicitly, others implicitly) mechanistic causation with causation in general; and, rightly believing explanation in terms of causation to be of the essence of science, have held that they must aim at mechanistic explanation or remain outside the pale. Under the influence of this delusion they have manfully hidden from themselves the obtrusive fact that men and ani-

mals are goal-seeking organisms, that the activities with which psychologists are concerned are in all cases purposive activities and can be explained only in terms of teleological causation. And this unfortunate bias is confirmed in many by an almost sacred horror inspired by the external teleological explanations of the theologians. The sterilizing influence of this blinding bias is manifest in all the history of P. E. Wundt claimed that his psychology was voluntaristic, and contrasted it with intellectualistic psychology. But if we look closely into the question—What did he mean by this?—we find he meant merely that immediate experience, the description and explanation of which is (he said) the whole task of psychology, is an interconnection of processes rather than a sum of objects. Throughout the earlier two-thirds of Wundt's *Outline* we find no trace of recognition of the teleological nature of human action. In the final chapters we find incidental mention of "purposive ideas," of "ideas of end," and of "purposive reflexes;" and on the penultimate page we discover the "law of heterogony of ends," which, we are abruptly told, "dominates all psychical processes." The frankly mechanistic associationism of the bulk of the book thus becomes faintly tinged with a suspicion of teleology.

If Wundt may be said to have hedged on the problem of teleology, the great majority of the psychologists of the laboratory have been mechanists, naked and unashamed. Yet of recent years the facts have been gaining a partial and grudging recognition in experimental research under a variety of disguises, such as "the influence of the *Aufgabe,*" the importance of *incentives,* of *determining tendencies,* of the *tendency to closure,* of motives, of goal-seeking, of *drives.* In fact, the experimental study of motivation is rapidly becoming a topic of central interest in the laboratories. Dr. C. R. Stone in a recent article lists fifty-two titles concerned mainly with sexual motivation in animals, and remarks: "Today, no more promising subject of investigation in psychobiology can be found than that of motivation." The experimental work of Dr. Kurt Lewin and his colleagues has been for some years directed fruitfully to problems of this type in the human subject. The same may be said of Dr. Charlotte Bühler who, unlike most of the workers along this line, frankly recognizes the purposive nature of the activities he studies.[12]

[12] In this connection two recent American books are important and significant, Dr. C. G. Warden's *Animal Motivation Studies* (New York, 1931), and Dr. C. E. Tolman's *Purposive Behavior* (New York, 1932).

This second great ground of the relative sterility of P. E. is, then, passing into history; and we may fairly hope that, in consequence, P. E. is entering the path of true and steady progress.

Yet the first difficulty remains and from it arise some serious secondary difficulties whose retarding influence is great at the present time. The impossibility of crucial observations gives scope to fantastic and extravagant doctrines, and allows them a long lease of life: for such doctrines, dogmatically propounded in striking sensational formulae, catch the public fancy and enjoy great popular vogue, totally obscuring from the public the sober and solid contributions of the more scientific workers. Thus in America the general public ignores psychology, while the catchwords of behaviourism and of psychoanalysis fall from its lips at the slightest provocation. Yet behaviourism may fairly be likened to the flat-earth doctrine, and Freud's pansexualism is to modern psychology very nearly as the theory of phlogiston was to early chemistry. Those outmoded doctrines were rendered untenable by crucial observations. What crucial experiments shall deliver us from these modern incubi?

I will conclude by venturing some remarks of a practical bearing. In re-reading Professor Boring's *History of Experimental Psychology,*[13] I am struck by the fact of the extreme youth of the authors of many works that rank high in the history. Berkeley and Hume were little more than schoolboys when they propounded their brilliant but absurd "idea psychologies." Bain and Herbert Spencer produced their principle treatises in their middle thirties. Wundt, far better prepared than most, began publishing in psychology in his twenty-sixth year. Külpe wrote his *Grundriss* in his thirtieth year. Dewey, his *Psychology* in his twenty-seventh. Thorndike made his classical experiment and fixed his all-too-simple principles in his twenty-fourth year. Titchener became head of a laboratory at twenty-five and forthwith began to force the Wundtian psychology on a long procession of students. The last case is particularly illustrative of the point I wish to make. If this ardent student had spent another ten or fifteen years in preparation for his task, working in biological laboratories and playing with children and madmen and savages and animals of all sorts, he might well have made a very much better contribution to his chosen field. For the promotion of

[13] The only serious fault I find in this excellent treatise is the assumption, implied in the title and in many passages, that experimental psychology is a distinct discipline which stands over against another to be known as philosophical psychology.

psychology requires (in a much higher degree than any other science) a mature mind, mature in two senses; mature with a degree of openness and wisdom which a young man, however brilliant, can hardly achieve; and mature in the sense of having made much varied and intensive study of many branches of science and philosophy.

Not only do the workers in the field of psychology enter it too early, but also, in too many cases, owing to its great difficulty, they leave it too soon; either they exalt themselves to administrative jobs as college deans or presidents or as ambassadors, or they desert psychology to become psychoanalysts or philosophers or distinguished educationists. Comparatively few stay the course, as men commonly do in the easier sciences. The result is that among the many workers in psychology is a lamentably small proportion of mature minds.

If all our would-be psychologists could first graduate in biology and afterwards in medicine, and then, before publishing or teaching in psychology, give a few years to practice among nervous and mental patients and a few more to field anthropology and natural history, the prospects of progress in our knowledge of human nature would be much improved.

PERSONALITY TESTS—WHITHER?

A. A. ROBACK
Cambridge, Mass.

Whether we accept as the pioneer of personality testing Jung, who as early as 1905 devised a series of association tests by which individuals were to be placed in a typological schema, or Fernald, the projector in 1912 of the first set of character tests in the form of questions designed to measure moral consciousness, we find that in the third decade of the present century, the psychological atmosphere is precipitate with the measurement of personality elements. I say personality elements advisedly, for not even the most sanguine investigator, to my knowledge, will, in the present stage of testing at least, go so far as to claim that he is actually measuring personality as a whole.

An Era of Measurement. This is the era of measurement. In view of the principle that everything which exists, exists in some quantity, *ergo* can be measured, and furthermore, in view of the premise that the more closely a study approaches the scientific ideal, the more frequently its results can be put into quantitative terms, the mathematical movement in psychology is natural enough. It was at first most conspicuous in the numerous reaction investigations of several decades ago, when the Chronoscope was looked up to as a sort of oracle, and perhaps also in the plethora of psychophysical experiments and discussions which were so zealously conducted in the laboratories and aired in the periodicals.

The intelligence test movement diverted the attention of the average psychologist from the experiment to the test, from the general law to the individual difference; at the same time it retained the quantitative interest and emphasis on formulae, which Binet himself, the innovator of the intelligence test, could not have foreseen. Nor, would he have relished it, if he had foreseen this aspect. That personality tests, which cropped up first of all in the form of association correlations, ethical questions, emotional tappings, and subsequently will and temperament searches, and motivation analyses,

all with a quantitative slant, would follow close upon the heels of the intelligence tests was after all to be expected; but could anyone have predicted, even as late as a decade ago, the avalanche which bids fair to sweep away from the foreground nearly all interests in American psychology, to the exclusion of personality measurement?

In 1927, when my *Psychology of Character* first appeared, the chapter entitled "The American Experimental Contribution" with its close to thirty pages seemed quite lengthy as a survey of the field; at present nothing short of a book would do the subject justice. If our modest ideal at one time was to test traits, one finds that this goal has long been reached or, perhaps better, over-reached. Most personality testers, for reasons which will be examined presently, have sidetracked the trait in order to measure attitudes and interests, opinions and prejudices, complexes and tendencies of all descriptions. In my *Bibliography of Character and Personality,* there are over 250 references under the quantitative rubric. Since this bibliography was published, the material has been at least doubled, and the measuring activity is becoming more intensified from month to month.

On general principles, this approach to the study of character and personality is gratifying. All scientifically-minded people must recognize that the subjective treatment of such an important field could not advance our knowledge. It is obvious we must have an objective mode of procedure, if for no other reason than to gain a modicum of uniformity, as for example in the experimental psychology of sensation and perception.

The Purpose of Personality Tests. In surveying the results of the personality tests, our first step is to *ascertain the purpose of the tests.* What do we expect the harvest to yield? This question has never been asked before, probably because the answer is invariably taken for granted; and it is taken for granted because the field of testing is scarcely ever viewed in perspective.

As to the question posed, since there seem to be four chief benefits accruing to society from personality measurement, we may say that testing the non-cognitive functions of man has been undertaken, although perhaps not deliberately, for a fourfold reason:

(1) To throw light on the general nature of personality and its mechanism.

(2) To place an individual in relation to others.

(3) To offer a body of sociological data with regard to the behavior of an individual in a group.
(4) To ascertain the best methods for training the young along moral and social lines.

It is evident that the first reason concerns theoretical psychology; the second bears on psychotechnics, or even applied psychology in the broader sense. To know something about the individual, whether he is emotional or prejudiced, dishonest or generous, is an invaluable guide not only for the employer, but also for the testee. Vocational guidance should be the first beneficiary of personality research, but psychiatry is no less beholden to it; even medicine and law can acknowledge a certain amount of indebtedness to personality testing, and educators theoretically may gain insight in understanding their charges through the revelations of such measurement. The third purpose is essentially a sociological one, while the fourth is decidedly to promote a pedagogical end, with an ethical (or religious) coloring.

Our next query is: Have the hundreds of quantitative personality investigations borne out their promises? Can the results of such investigations be placed on a par with the results in the intelligence testing field? Do we in consequence of such tests know more about the human psyche both individually and in its social milieu?

This issue can be dealt with more satisfactorily by reverting to our above-mentioned reasons for administering personality tests (tests to be understood in the widest usage, so as to include questionnaires, interviews, ratings, etc.)

To begin with the first aim: it is questionable, in my eyes, whether the theoretical status of personality has been at all grasped because of the quasi-experimental activity in this field; and small wonder. We can hardly expect to understand the architecture of a magnificent structure by examining the bricks, mortar, stones, and pillars of the façade. If consensus of opinion, uniformity and objectivity alone were our desiderata, then certainly personality testers have run afoul both of each other, and of the mark. The greatest agreement is in the negative direction, e.g., in a tendency to discredit the existence of traits and to break up personality into so many habits and acts.

Results Must Be Referred to a Standard. Has the personality tester succeeded in rating a given individual with reference to others? This question may be answered "yes and no." *Every rating presup-*

poses a standard, but testers are often so busy with correlation coefficients, standard deviations, tetrad difference criteria, probable errors, corrections of formulae, that the standard is lost sight of. It cannot be gainsaid, nevertheless, that the test, if properly conceived and scientifically devised, is of considerable use in diagnosis or even in prognosis. The typologist, pure and simple, unless he possesses an unusually intuitive mind, can scarcely hope to reach the same definiteness and decisiveness as the personality tester achieves by the use of several different batteries of tests. On the other hand, the latter must reckon with different schemes of types or traits, or else his figures will be meaningless. Even in such a rectilinear scale as the intelligence quotient, the mere graduation of the line into 120, 100, 90, 70, etc., *would possess no significance if the figures did not correspond to definite mental levels,* recognized as such in practical life.

An Unexpected Cache. Probably the greatest comparative service rendered by personality measurement has been in the furnishing of interesting data of a sociological nature. This is the unexpected ore discovered in the mines of personality. In the opinions, beliefs, attitudes, and interests of students, to take one group only, we find so many oddities and paradoxes that some of the accepted views in sociological circles will bear revision. It is here that we strike a meeting ground between psychologist and sociologist, and possibly the latter's growing interest in the psychology of personality is largely due to such findings.

Finally, the pedagogical *motif* in the quantitative investigation of personality, it is to be feared, does not fare so well as the sociological groping. Curiously enough, the sociologists received a windfall when they were least anticipating it, while the efforts made by religious bodies to develop the morals and morale of the youth of the country have thus far yielded fruit only in a *methodological* way, which, in itself, is of course no mean asset. Incidentally, the fact that religious organizations have financed quantitative investigations of personality conducted from a behaviouristic angle, as the stimulus-response terminology often shows, is a sign of the times.

II

It is needless to dwell on the shortcomings of most of the scales, batteries, ratings, questionnaires, and personal data sheets; for by this time the criticisms are fairly well known in characterological circles: the unreliability of ratings, the subjectivity of self-ratings, the inadequacy of information tests as guides of behavior, the in-

consequentiality of some of the habits (traits) tested, the inconsistency between what a testee says he would do under the circumstances and what he actually does do, the incomparableness of artificial settings to life situations, and several other such vitiating factors. I have come across tests which incorporate at once three or four of the objectionable features just referred to. One test purporting to give us a clue to tactlessness through the question-answer method lists apparently among tactless acts assault and battery and even murder. This is symptomatic of the omnibus mind bred largely in the United States and intent upon bringing out statistical figures regardless of the incongruity between the correlates.

A Colossal Venture. Instead of examining the minor pieces of research, it would be more to the purpose to take as our instance the most representative of all such investigation, the Character Education Inquiry which was conducted on a gigantic scale by Professors Hartshorne and May, with their centre of operation at Teachers College, Columbia University. The staff of workers engaged in this undertaking, I understand, numbered forty besides the principals and teachers of schools who coöperated with the investigators.

The inquiry lasted five years and was financed by the Institute of Social and Religious Research. Eleven thousand children were tested in various situations for honesty alone. The results of the whole inquiry have been incorporated in three volumes of over 1700 pages. The enterprise which was carried out with all the technique and efficiency which makes American machinery sought after in every nook and corner of the civilized world, is reminiscent to a certain extent of the organization in the production of a major cinema. We are dazed by the hundreds of tables and figures, scattergrams and histograms, correlation formulae and appendices. If Professors Hartshorne and May had done nothing else than to develop a methodology of character testing, they already would be entitled to a place in the forefront of American characterology. Among the subsidiary problems which they probed are the relation of deceit to age; the relation of deception to intelligence; the socio-economic background, as well as the cultural background, as a factor in honesty; differences of nationality and religion; the honesty reactions of siblings, etc.

It was with some gratification that I noticed that the second volume, *Studies in Service and Self Control,* dealt with the implica-

tion of my own doctrine of inhibition as the core of character; and it is in this phase of the inquiry that the most important and most clear-cut results seem to have been obtained.

The Case for Specificity. The nubbin of this whole prodigious labor appears to be that conduct is specific, and that there are no traits. This *motif* is repeated many times in each of the volumes. Since the children who cheated did not cheat in every test, e.g., some copied, while others would use a key, still others would fake a solution to a puzzle, while not a few would doctor up their answers, or since some would cheat in arithmetic tests and not in completion or information tests, while with others the converse would be true, or since some would steal when the temptation was supplied them, while the majority of cheaters refrained from taking money not belonging to them, it was concluded that neither honesty nor dishonesty was a trait, but that in every case, we are dealing with specific attitudes and habits.

"Our conclusion, then, is that an individual's honesty or dishonesty consists of a series of acts and attitudes to which these descriptive terms apply. The consistency with which he is honest or dishonest is a function of the situations in which he is placed in so far as

(1) these situations have common elements,
(2) he has learned to be honest or dishonest in them, and
(3) he has become aware of their honest or dishonest implications or consequences."[1]

Before we go any further, let us note how the authors stress what they designate as consistency (and what should properly be called uniformity or constancy) and the community of elements in the situation. The attitude or act is, in other words, a function of the situation. Due cognizance of the motive of the child has not been taken except in a separate sheet on one occasion when it was asked what it was that induced cheating in the dishonest. Naturally the answers of the comparatively few "honest" cheaters, i.e., those who admitted copying—answers such as "test too hard," "Others cheated," "Lazy," "Felt like it," "Test Unfair," "Too many chances," "So class would win"—do not give us an insight into the working of the child's mind.

One may go a bit further and express misgivings as to whether a child in grammar school is a fit subject for a character test, the whole weight of which rests on the problem of motivation. For a

[1] H. Hartshorne and M. A. May, *Studies in the Nature of Character*, I, 380.

genetic study of character, the school is undoubtedly the best field of operation. It will also be recognized that the purpose of the inquiry was largely pedagogical and religious. Moreover, the facilities offered by the schools and the docility of the children are conditions not obtainable in factories, plants, and offices.

Rôle of Motives and Viewpoints. At the same time we cannot shut our eyes to the fact that children of eight or ten certainly have not yet had the chance to find themselves. Many of them do not understand the significance of honesty; and their inconstancy is, in a great many cases, due to the fact that they are groping. Situations count, but motives—and by motives I do not mean merely reasons but the *whole mental state preceding the act*—count for a good deal more. Children will cheat in one test and not in another, because

(1) they like one subject more than another
(2) they are more backward in some studies
(3) they have more opportunity to "make a better job of it"
(4) they feel arbitrarily disposed, or "the spirit moves" them.

The lack of constancy does not necessarily have to be explained by the doctrine of specificity. In fact none of the results, if adequately analyzed and interpreted, militates against the common sense notion that there are traits governing a multitude of acts, which, though very unlike one another, are called forth by a similar motive and attitude.

If, as Professors Hartshorne and May contend, the habit and the situation are the only factors to be considered in honesty and other traits, then surely since the neural mechanism involved in copying and in doctoring up answers cannot be the same, the habit cannot be a common one, and if so, the whole project of character education is futile, because *training would have to be applied to every individual set of neuro-muscular processes involved in every single type of act.* And if the reply to this objection is that the common elements in all the specific acts are to be educated in the desirable direction, the concession is obviously made that *there is something which is not specific,* and that that something will have to be discovered before the training could be begun. Furthermore, it is more likely that this common element will be a central factor rather than a mere common path leading to an effector. In other words it will be a trait, in which the motive and the viewpoint of the individual will be the dominant features.

When a boy in school who is guilty of some mischief in the ab-

sence of the teacher rises at the time the culprit is asked to make himself known, he is actuated not so much by honesty as by a sense of bravado and fear of the consequences, particularly by the opinion of his classmates. This is a matter of honor rather than honesty. There is a different congenital tendency affecting the act, just as when a dying gangster in refusing to divulge the name of his assailant acts in accordance with his "code of honor," although almost every other day he may be guilty of breaking all the commandments of the Decalogue.

Conventional Concepts Without Psychological Foundation. How we can evaluate a certain act and call it cheating or honesty without knowing something about the point of view of the individual in question is a little difficult to understand for one who is not a legalist. *The analysis of a concept must precede the investigation making use of the particular concept.* Honesty is a virtue which exists but rarely, and what is worse, very frequently the term is applied superficially under a conventional purview.

Does the workingman who takes out his family of six for a trolley ride and pays only five fares think that he is cheating the officials of the transportation company who receive individually more in one week than he does for a whole year's hard work? We say he is dishonest, because in the eyes of the law, his act is culpable, but it is possible that there is a law of equity which judges him differently. Similarly the banker who floods the exchange with bonds for a country which he knows to be on the verge of bankruptcy does not consider himself dishonest, although in reality he is more than dishonest; nor are senators, in inflating their disbursement sheets at the expense of the tax payers, any different from common thieves, though the neuro-muscular acts are entirely different. The common mental element which connects the two types of behaviour is the knowledge in both cases that the *deed is depriving others of necessities or comforts which are theirs rightfully,* that they are benefiting unwarrantedly by other people's energy. That is what makes the act dishonest—the fact that someone is using another for his personal greed or pleasure without the other's knowledge, consent, or sanction, and without due compensation. Not an act but a principle is fundamental here.

Regulative Principles Produce the Acts More than the Situation or Habit. We must come, in the last analysis, to the conclusion that not a myriad of acts, not a thousand and one independent habits and

attitudes constitute character but a few regulative principles curbing the instinctive tendencies through the function of inhibition. In the characterial hierarchy, the acts, it is true, are integrated into habits, but then the habits range themselves into traits, and the traits into principles. The greater the number of elements on a lower level which can be integrated into a higher complex and the more rapidly this organization takes place, the firmer and the sooner has a child's character begun to establish itself. Every intelligent child will learn more from the formulation of a principle as to what type of behaviour is to be avoided than from a whole manual of individual and apparently unrelated rules. Whether these rules, taught in childhood, can be applied to adult situations is another question which cannot be so easily settled, and which adds to the complication of the specificity doctrine. Principles, on the other hand, broaden of their own accord in the course of maturity so as to subsume a variety of situations. The authors make a point of saying that to those who understand the rule of addition, the rule of subtraction must still be taught. That this is actually so may well be questioned. At any rate, it is here that we can perceive a fundamental difference between a rule and a principle.

Far from accepting it as a "fair conclusion" from the experimental data "that honest and deceptive tendencies represent not general traits nor action guided by general ideals, but specific habits learned in relation to specific situations which have made the one or the other mode of response successful,"[2] I should rather consider the results as favoring the view that acts emanate from traits, and traits are a function of constitutional make-up (instinctive urges) and regulative principles.

In his attempt to steer a middle course between the omnibus and the hierarchical points of view, Professor Murphy asks rather pertinently "If the generalists were entirely right, it is hard to see why one shall need such a very large sampling of specific evidence in order to make a respectable predication in the future. They ought in fact, if completely logical, to admit that an individual having had full opportunity to manifest his honesty in a certain situation could be depended upon to show the same degree of honesty in some new situation."[3]

[2] H. Hartshorne and M. A. May, *Studies in the Organization of Character*, volume III of *Studies in the Nature of Character*, p. 372.

[3] G. Murphy and F. Jensen [with a supplement by J. Levy], *Approaches to Personality*, pp. 385-386.

Rules and Exceptions. As a matter of fact that is exactly what happens to be the case in everyday life. The convicted forger will not be employed as a cashier or even salesman; the slovenly matron will not find it easy to secure a position as a cook or maid in a well-conducted institution; a deserter from the army will scarcely be intrusted with a commission of importance even after he has returned voluntarily and become reinstated. It is true, however, that we cannot always predict an individual's behaviour from his previous conduct. An oppressive foreman may be a devoted and kind-hearted husband or father; a model boy in high school may turn out to be a "blue sky" bond salesman when he gets out of college, etc.

This puzzling feature about character is only natural.

In the first place, our expectations may exceed our rights, considering the scantiness of our knowledge and the chaos of our conceptions, e.g., cheating is a term applied to stealing, copying in school, doctoring experimental results, and extra-marital sex activities. In reality there is very little which is physiologically and psychologically common to all these types of conduct; and a knowledge of these instinctive urges is most essential. Acquisitiveness and greed are not in the same line as glory-seeking, and both are a bit distant from the pleasure principle manifesting itself in sexuality. The whole mosaic of motivation, as well as the point of view and regulative principles of the individual, must be considered in the predicting of behaviour. Political grafters will not bother with a too small amount, or where the risk is too great, but one can always depend upon their rising to the occasion when a tidy sum is to be pocketed. If there are exceptions, then the exception can be explained. We must surely make allowance for mental conflicts, compensations, fears, physical conditions, and moods in understanding the apparent inconstancy of an individual. After all that is where psychology is different from physics. Matter cannot determine itself; man exercises a self-regulative influence. Even inert matter will present difficulties at times. The static of the radio, the problems in television will occur to one immediately. But the reason why we can predict the behaviour of physical and chemical phenomena is that we *control* the conditions. If we could control in equal measure the mental conditions of the individual we are "sizing up," the prediction would be just as infallible as in the case of physical entities. We must remember that even biology has its freaks and sports that baffle all efforts at explanation.

A Doctrine Which Defeats Its Own End. To be sure, the study of character and personality has not reached the high level of reliability enjoyed by physics and chemistry, but we cannot force the issue by assuming a mechanical attitude toward the whole subject. The didactic object of character research is especially jeopardized by a doctrine which, *ab initio,* spells defeat.

It is well that Professor G. B. Watson, who by virtue of his training showed some leaning to the omnibus school, has come to see the footlessness of a testing which deals with separate acts and habits. In his comprehensive recent survey of character and personality tests he writes:

1. Character testing may be improved by better characterology. We can test almost any conceivable trait, today, but are far from testing character.
2. Better characterology will give us better units for study. The inadequacy of the ethical "trait" has been demonstrated too often to need further study. The improvement of character testing will mean testing in units which are the same in their dynamics, their inner structure, in laboratory, home, school, office, or senate.
3. Better character tests will not be content with the measurement of behavior in a few situations, but will present experimental evidence that the pattern really is identical in a wide variety of social and material environments. This is validation.
4. The reliability of tests will be improved, not so much by mere increase in length, as by more accurate insight into the behaviour involved, so that errors of misinterpretation and mistaken expectations of consistency are removed.[4]

When institutions will realize that in order to neutralize the particular laboratory bias or methodological prepossession of a certain investigator, it is advisable to see to it that the directors of a given piece of research are not like-minded, to begin with, and have not come under the same training influences so that in interpreting and analyzing the results one point of view receives as much consideration as the other—then, and not before, shall we begin to see light in the complicated maze of character and personality tests. Until then we shall still be obliged to ask "Personality Tests—Whither?"

[4] G. B. Watson: "Tests of Personality and Character," *Review of Educational Research,* II (1932), 257.

THE AFFECTIVITY OF KRETSCHMER'S CONSTITUTION TYPES AS REVEALED IN PSYCHO-GALVANIC EXPERIMENTS

ELISABETH ENKE

Neuropathic Clinic, University of Marburg

In setting up his classification of human types according to character and physique, Kretschmer was simultaneously laying the foundations of the theory of temperaments. The more recent studies in experimental psychology carried on by his school aim at the elaboration and extension of this theory. In the experimental study of the sensor, associative, and psycho-motor functions of the constitution types it was observed that the results of the performances of the several types were largely affected by the kind and degree of their affectivity. [What we call the temperament of a person is simply that general tenor which characterizes the entirety of his life; together with his impulses, temperament forms the constitutional basis in the formation of character. The result mentioned above, which was discovered in the course of sensor and psycho-motor experiments as well as in studies on the psychology of thought, thus confirms the view taken of the importance of temperament as a factor in the total personality.] In these experiments it was impossible to isolate the factor of affectivity; one of its components, the responsiveness or capacity for internal nervous excitability, could not be perceived, still less measured, in the course of experiments, while stimulative components like attention, strength of expression and energy could be observed but naturally not apprehended in isolation.

It is known that slight variations in feeling manifest themselves in slight variations of innervation; accordingly, in order to measure affectivity all such methods might be employed which make it possible to measure such variations as are reflected in pulse, breathing,

blood pressure, and the electrical resistance of the body. The most suitable method for this purpose seemed to W. Enke to be the psycho-galvanic reflex phenomenon as first described by Veraguth in 1906. The phenomenon relies on the fact that an extremely weak galvanic current in whose circuit the subject of the experiment forms part passes through the body in varying strength according as the person's affectivity is exposed to stimuli of a physical or psychical nature. By means of a mirror galvanometer, which also forms part of the circuit, the variations in the current can be read off directly on a scale. The physico-physiological factors of the phenomenon appear to be due to the fact that variations in the resistance to the electric current in or on the surface of the body are caused by functional changes in the sweat-glands resulting from affective stimuli. The view has always been held practically universally that at bottom the phenomenon is of a psychological nature, being, in fact, the expression of affective reactions; and this view is supported by the latest investigations of the psycho-galvanic reflex made by American psychologists as well as by experimental studies on somato-affective changes made by Wittkower and Fechner at the Charité in Berlin. The experiments agreed in indicating that the psycho-galvanic reflex measures the affective variations of a person with extraordinary accuracy.

The experimental method pursued in the psychological laboratory of Kretschmer's clinic closely resembled that of Veraguth as described above. The subject of the experiment lies in a dark room which is kept as quiet as possible and reclines on a couch in such a way that the apparatus is invisible to him. He forms part of the galvanic circuit by means of two zinc electrodes of the kind normally used in diathermy. These are attached to the arms above the wrists and are kept fast by a rubber band in order to eliminate as far as possible any innervation interference on the part of the subject. The times, which are taken from the mirror reflections by means of a stop watch, are noted down in writing at maximum intervals of five seconds. Before the experiment begins the subject is told that he need not do anything himself and that "nothing would happen."

Experiments were made altogether on a hundred healthy persons of either sex consisting equally of pyknic, athletic, and leptosomatous constitution types aged between seventeen and sixty, drawn from the

most diverse professions and having the most diverse education. The types were chosen as "pure" as possible.[1]

In order to discover how the affectivity of the various constitution types is manifested in the psycho-galvanic reflex experiment, it was essential to select relatively neutral stimuli which should be as "indifferent" as possible, i.e., such as could not impinge upon any individual complexes. The psycho-galvanic phenomenon is peculiarly well adapted for this purpose, because the so-called experiment of rest itself is a stimulus of the desired kind. The experiment of rest simply shows us the affective reaction of the subject of the experiment towards the introduction of the experiment itself, i.e., the attaching of the electrodes. Externally this stimulus simply consists in the negligible manipulation connected with the attaching of the electrode; internally, however, it places the person in a state of intensified anticipation and increased attention and suffices to reveal considerable differences in the general affective disposition and the nervous internal stimulability of the constitution types.

When the variations in current caused by the purely mechanical act of attaching the electrode had ceased, the behaviour of the current was observed and noted for a quarter of an hour during which the subject of the experiment reclined at rest. During this quarter of an hour, a state of internal affective rest was attained on an average in nine minutes by the pyknic and athletic types, and after fourteen minutes by the leptosomatous type. The earliest moment at which rest was reached was after the lapse of two minutes with pyknics and athletes, whereas it amounted to ten minutes with the leptosomes; forty-five per cent of the leptosomes had not reached affective rest at the end of the period of rest, i.e., fifteen minutes after the electrodes had been attached. This was the case with only 3.5 per cent of the athletes, and with six per cent of the pyknics. The total range of the curve of rest, i.e., the difference in the height of the curve between beginning and end was 161 mm. with the leptosomes, 104 mm. with the athletes, and 80 mm. with the pyknics. The curves thus show characteristic differences which clearly appear in the copies (Figures 1 and 2) of the rest-curve of a pyknic and a leptosome, although the curves are reduced in the ratio of five to one.

To sum up, the result of the experiment is to show that the

[1] The pyknic type is well rounded, thick set, with short limbs; the athletic type has strong bones and is muscular; the leptosomatous type is slim and has slender limbs.

nervous irritability which we describe as affectibility is considerably greater and also more lasting with the leptosomes, while pyknics and athletes are less irritable and attain a state of rest much more rapidly. The result is a complete confirmation of Kretschmer's theory of the essential difference between the schizothymic and the cyclothymic temperament; it also agrees with the result obtained by the above-named investigators who applied the psycho-galvanic reflex experiment: "The more harmonious the subject of the experiment, the less reactions does he show; the more internal conflicts and instability he possesses, the more reactions does he display." And indeed the typical healthy pyknic is a harmonious individual who is relatively free from severe and lasting internal struggles, and is so precisely because of the sound equilibrium of his affectivity which is so clearly displayed in the psycho-galvanic phenomenon. On the other hand, the schizothyme is a man full of internal tension and easily irritable, whose reactions to external stimuli frequently are disproportionately great and whose iron self-control is frequently broken through by explosions of irritability and exaggerated outbursts of feeling. The psycho-galvanic experiment clearly reveals the enormous affective burden permanently borne in every-day life by the schizothyme, since even his reactions to the moderate stimuli of the rest experiment are so strong and lasting. Kretschmer compares the schizoid to a closed house behind whose darkened front feasts are celebrated which remain invisible and inaudible for others, and the rest-experiment, with its wavering beam of light on the scale, provides a striking parallel to this comparison. The curve of rest for the leptosome in Figure 1 is that of a typical schizothyme behind whose iron calm and apparent coldness nobody (not even the experimentor, who happened to be an old acquaintance) would have suspected the lively excitability and affective restlessness revealed in the numerous powerful and spontaneously recurrent vibrations lasting throughout the quarter of an hour's period of rest. Moreover, the general mental situation of the person in question was not abnormally surcharged with conflicts or abnormally restless, as was definitely shown by subsequent investigation.

None of the leptosomes grew tired during the experiment, still less did any fall asleep, whereas of thirty pyknics two, and of thirty athletes one fell asleep at the end of a few minutes. At the end of the experiment several pyknics declared of their own accord that they

would like to repeat the experience because it was so restful, almost allowing one to go to sleep; whereas when the leptosomes were questioned afterwards whether they had felt sleepy during the experiment they replied almost without exception, with a certain indignation: "Why, it's so exciting." [Although in fact nothing whatever happened during the quarter of an hour]. "Nobody could possibly think of sleeping."

Thus the experiment clearly reveals the opposition between the schizothymic and the cyclothymic affectivity of the leptosomes and the pyknics respectively. The former display a maximum of irritability and the state of stimulation is extremely lasting; frequently it must be described as a kind of spasm or perseveration of the emotion; the latter do not react violently to stimuli and quickly return to rest, i.e., the affective range quickly returns to absolute rest. The behaviour of the athletes in this experiment stands midway between leptosomes and pyknics. On the one hand they are easily stimulated, but on the other they attain rest as quickly as the pyknics, and often spontaneously describe their relatively calm mental state during the experiment after the test is concluded. Thus they show themselves to be something other than "pure" examples of the schizothymic temperament, while it would be equally untrue to describe them as simple mixtures of cyclothyme and schizothyme, as will be apparent from the results of experiments which we will describe later on. On the contrary it would appear that as a result of these studies in affectivity, we are justified in differentiating within the general category of schizothymic forms. Such a differentiation was indeed felt to be necessary by Kretschmer and many of his followers.

The second test of affectivity which immediately followed the rest experiment contained a stimulus which consisted in instructing the subject to think of one of the numbers between one and nine, and to retain it in mind. He was further told that at the end of the experiment, i.e., after the experimenter had counted slowly and in a loud voice up to nine, he would be able to tell the figure thought of. These instructions induced a state of concentration and affective tension. Of course the object of the experiment was not to read thoughts, although in fact it was eventually found that in seventy per cent of the cases the figure thought of was correctly determined; the real purpose of the experiment was, again, to discover the degree and duration of the affective stimulabil-

ity of the several constitution types. It was found that with this kind of stimulus the nervous internal excitability was greatest with the leptosomes. As appears from Figures 3 to 5 the leptosomes react to practically every number, although on the average the reaction is not as strong as for the number thought of. Hence their curves are difficult to read; as a rule the reaction to the figure thought of is not apparent at first glance. On the other hand the pyknics (see also Figure 6-8) remain quiet except for the first figure and those immediately preceding and following the figure thought of; thus they are not in the state of affective tension which can be easily increased by any "indifferent" stimulus, e.g., the mention of numbers, wherein they differ from the leptosomes. Accordingly the sums of the range of the indicator in this experiment amounted to seventy mm. with the leptosomes and to thirty-eight with the pyknics. The athletes, with forty-six mm., once more hold an intermediate position.

Once affective calm had been definitely established, the expectation experiment was followed by an experiment based on sense stimuli, and the affective behaviour of the constitution types with regard to various slight sense stimuli was tested. The stimuli consisted of a pleasant and an unpleasant smell, a slight prick and an unexpected auditory stimulus. The objective magnitude of the stimulus was, of course, the same for all those experimented upon. The olfactory stimulus was easily brought about by opening and shutting a bottle of eau de Cologne and a tube with a substance containing sulphuretted hydrogen; the prick was applied by a special small apparatus and the auditory stimulus by firing a child's pistol in each instance at the same distance from the subject of the experiment and invisibly to him. A new stimulus was only applied after affective calm had followed the application of its predecessor.

Here again the differences between the constitution types were revealed in the tension of expectation. While the person conducting the experiment approached the object of the experiment in order to apply the stimulus, the affective variations up to the moment of the application could be read by an assistant. The total range of the indicator due to expectation in the first three sense experiments was roughly twice as great with the leptosomes as with the pyknics, a result confirming that reached in the rest experiment and the expectation experiment proper.

In most instances the affective reactions to the pleasant smell

were stronger than those to the unpleasant smell, partly perhaps because the former was the first, and for that reason the most powerful stimulus of the series. At the same time the range of the indicator once more showed important differences between the constitution types; the relative figures for leptosomes, athletes, and pyknics with the pleasant smell were 33, 18, and 17 mm. respectively, and with the unpleasant smell 26, 19, and 12. The leptosomes belonging to the schizothymic group as well as the athletes here displayed a stronger affective reaction than the pyknics. The figures for the time elapsing before a state of rest was reached after the reaction show that the reactions are also more lasting than those of the cyclothymic pyknics (Pleasant smell 101, 93, 61, unpleasant smell 87, 63, 54 seconds).

The relation between the figures was different for the slightly painful stimulus consisting of a prick. Here the pyknics showed the strongest and also the most lasting reactions. The average range of the indicator with them was thirty-six mm. as opposed to twenty-eight mm. with the leptosomes and twenty-two with the athletes. Affective calm after the stimulus was reached on an average in 121 seconds, whereas it was reached in sixty-one and fifty-four seconds after the olfactory stimulus. After the experiment numerous leptosomes were surprised when told that the stimulus was supposed to have been a painful one. It had not entered their consciousness as such; some described it merely as an "unpleasant touch," although, as already mentioned, the objective magnitude of the stimulus was the same in each case. The pyknics on the other hand reacted to the application of the stimulus with exclamations of fear and attempts at evasion. Afterwards they almost without exception referred to the "pain" and indeed it was they who called the stimulus of the experiment painful. The numerical result seems particularly interesting because it confirms differences which had hitherto been laid down by Kretschmer in a merely descriptive manner. The pyknic is the type which attaches particular importance to bodily well-being and enjoys eating, drinking, and all the material pleasures of life. A disturbance of these vital feelings will therefore irritate him more powerfully than it will the schizothyme, who tends to endure hardships and to support hunger, thirst, pain, and cold up to a fanatical degree of hardening. The self-discipline of the schizothyme frequently consists in a restriction of his vital requirements, a restriction which his general disposition allows him to impose more easily

because, as our experiment shows, he is less sensitive towards disturbances of his vital feelings.

If the experiment with a painful stimulus emphasizes the one pole of the affectivity of the schizothymes, viz., their insensibility, the numb or "chilly" component in their disposition, the subsequent auditory stimulus, which amounts to an attempt to frighten, throws light upon the hyper-sensitive counterpole. Here the leptosomes proved particularly sensitive and nervous. Figure 9 shows the extraordinary strength of the reaction of the leptosome to this stimulus, although his affective reaction to the painful stimulus had been slight. He thus differs from the pyknic reaction which, as Figure 10 shows, is characterized by considerable sensitiveness towards the painful stimulus. With the leptosomes the range of the indicator after the pistol had been fired was, on an average, twice as great as after the other stimuli. The athletes, too, with a range of thirty-eight mm. displayed their greatest average reaction as a result of the shot, whereas the reaction of the pyknics (twenty-nine mm.) was very much less than after the painful stimulus.

The experiment also displayed very clearly the capacity of a pyknic for discharging his affects proportionately to the stimulus, in other words, for a psycho-motor discharge. A number of pyknics reacted to the shot with loud cries of fear; but at the same time no strong affective variation was visible in the galvanometer, and affective calm was soon reëstablished. In one instance a pyknic woman fifty years of age was sound asleep thirty seconds after the shot. Distinctions between the constitution types such as we know them from everyday life were here experimentally confirmed. The impossibility of mutual understanding between schizothymes and cyclothymes becomes perfectly intelligible. The self-control, the external calm, and apparent coldness of the former, which is partly due to their incapacity for immediate affective reactions outwards, is as incomprehensible to the pyknics as the liveliness and the wealth of their affective display frequently appears unnatural and exaggerated to the schizothyme.

In strength of reaction as shown in these sense stimulus experiments, the athletes approximate the leptosomes. But this does not apply to the period of reaction, i.e., to the time elapsing after each individual stimulus before the galvanometer has reached its maximum height and has thus registered the maximum of affective

excitement in the subject person. The sum of the reaction periods in all the stimulus experiments was approximately identical for pyknics and leptosomes, fifty-five and fifty-six seconds on an average; on the other hand with the athletes it averaged sixty-two seconds. This result probably reflects the fact that the athletic mind is slower to respond to affective stimulation; it has a greater viscosity in its affective currents, such as is frequently characteristic of the athletic type in ordinary life.

With regard to the time of reaction, i.e., the speed of the affective reaction, the athletes thus differ from the typical schizothymic personalities as well as from the cyclothymic pyknics. Further investigation is necessary before it can be established to what extent a qualitative difference of the affects exists as between the athletes and the other two groups. According to results hitherto obtained, the athletic group appears to have psychically a somewhat isolated position within the schizothyme group.

On comparing the reactions of all the male with all the female subjects without regard to constitution type, it was found that in the rest experiment the reactions of the two sexes were identical. In the expectation experiment the sums of the magnitudes of reaction were the same; with women the percentage of correct guesses regarding the figure thought of was greater than with men (eighty-six per cent as against sixty-seven per cent.) This may be due to the greater suggestibility of the female sex. The degree of affective response to external stimuli appears to be somewhat greater with women than with men, and accordingly the average values in the sense stimulus experiments are throughout greater with female than with male persons. On the other hand a comparison of the numerical results obtained with male and female persons in no case shows contrary reactions. Differences in the affectivity of the sexes are thus only a partial matter of degree; there is no qualitative difference. Qualitative differences are a matter of constitution.

Received March 15, 1932.

CHARACTER AND PERSONALITY AMONG AMERICAN INDIANS

MARY AUSTIN
Santa Fe, New Mexico

In taking stock of the mental processes of American Indians one has always to keep in mind the salience of primitive processes, in the emergence, above the rationalizing tendencies, of what is generally known as subconscious mentality. There can be no manner of doubt that in the normal life of primitives there is a preponderance of the sort of experiences which are described as hunches, intuitions, premonitions, clairvoyance, prognosis through dreams, visions and symbolism, and that these experiences lumped together as "Medicine" are generally more trusted by true primitives than any of the processes called intellectual. The farther back we go in the exploration of the human mind, the more we are convinced of an antecedent capacity for "dreaming true," that is, for dreams that enmesh tomorrow's incident with today's event.

Among the least successfully organized tribes the possession of such "medicine" traits are the true basis of leadership: direction of war and hunting expeditions go to the one whose "medicine has been good," or whose dreams are most significant. In the more highly organized groups, leadership devolves upon a shaman, a cacique, or high priest with a reputation for sustained "medicine power." Among the Pueblos, such a cacique promptly selects one or more—usually four—acolytes whom he instructs, and one of whom he finally designates as his successor on the basis of his subconscious capacity. Often these men possess high spiritual endowment, and indeed, the lesser priesthoods and headships of sacred societies are allotted on the same showing of qualities, which often include the gift of group hypnotism and quiet extraordinary clairvoyance. Among the Navahoes near Newcomb there is a "medicine" man who has been able clairvoyantly to trace the whole process of a theft which took place on a white man's premises several days earlier, and to follow it to the cache where the stolen goods were concealed.

Along with this emergence of subtending mentality there is also

among primitives a pronounced emergence of genius processes, by which creative power is more widely distributed among them than among us; so that there are few primitives who can not make some creative showing, in the ability to compose a song, a poem, a dance or a tale, a design, a drawing, or other creative work which will have, even by civilized standards, high artistic values. That genius in Indians extends to administrative and statesmanly capacity, there has been ample proof in the characters of Red Jackert, Gedwense, and Sitting Bull. And all this is so definitely a primitive characteristic that I have not been able to get on with a study of Amerindian mentality without arriving at explicit notions of what the genius process is—the emergence of an experimentally acquired ancestral habit, by which creative capacity is passed on to succeeding generations without the necessity of being rationally reacquired. In making judgments on individual Indians the leading character traits will be found generally to lie in the direction of the genius capacity. Most of the reactions of superior and inferior, of self-love, of ambition and dominance group themselves around it. It is, I am convinced, the leading item governing the "hunger vision," which is the central experience in the selection of the personal totem. A youth of inherent military genius will tend to discover a vision of a wolf, an eagle, a bull buffalo; one of a more social or spiritual tendency will hear a voice or experience a symbolic incident. A youth who never attains a hunger vision will likely prove to be without any dominating drive.

It is around the nature of these sub-conscious emergences that individual character tends to be built. What we have to bear in mind is that the whole tradition of them is empiric; certain practices and behaviours tend to foster such emergence and are encouraged for that purpose—prayer, fasting, continence, silence, ritual acts, singing, and dancing. In short, the preparation for the genius flash affords almost the only discipline of primitive life. In accordance with the intensity of his desire for a particular experience, the individual Amerind directs his practice, which is to some extent governed by the tribal concept of the good life, to be notable, to be heroic, to be generous, to be dependable, an ideal which almost never includes a desire to be dominant over material things, or, if so, in pursuance of a more urgent desire to be found generous, a liberal giver. It is the existence of this generally wide spread practice of interior discipline which gives to Amerind character its poise, its

balance, and its quality of resistance to the inroads made upon it by the thrusts of alien influence and incident.

As I have said, it is the genius emergence which dominates in the selection of the particular discipline. A man with a gift for song making will put himself to the discipline of collecting and memorizing tribal songs, to the discipline of maintaining the brooding silence followed by emotional release, out of which new songs arise; one with a gift for healing will memorize cures, and walk apart in that state of complete quiescence of the intellect which permits the secret quality of plants and minerals to pierce his consciousness; one with the gift for tracking either game or the human quarry will accept the discipline of concentration and meditation upon the traits of the quarry which enable him to identify himself with it at any distance. Some of these disciplines have marked traces of superstition, but many of them are psychologically so sound that I am making a careful collection of them. What they all imply is an explicit, outgoing motion of the under consciousness, an emergence of that motion into the plane of active consciousness as a thing done—subconscious become conscious not as an emotion but as an activity. They include, of course, many adventitious aids, such as drum beating, rhythmic motions, smoking, the handling of amulets and fetishes. But the point is that they all involve discipline, often painful and arduous. And it is the loss of this traditional discipline out of tribal life which is the source of the degeneracy of Amerindian character following on Americanized methods of education.

Any one in a position to note the alterations in poise and self-respect which have come upon the Pueblo tribes with the recent release of the genius process in them through admitting their native arts to the list of acceptable practices; and the superior resistance of the Navahoes to White degeneracy, which results from their greater freedom from White imposition, will not require any further comment.

Few of these disciplines imply any purely intellectual exercises other than the training of memory, which is often extensive, involving in the case of medicine men the retention of thousands of lines of song and ritual, the slightest error in which invalidates the whole ceremony. But mere memorizing without the accompanying exercises of concentration, meditation, and abstraction is of little avail.

Another influence the effect of which on Amerindian character is pronounced is the discipline of completely accepted group life, in-

volving as it does the ritualizing of ordinary every-day behaviour and the necessary suppression of individual idiosyncrasies. The unavoidable absence of privacy, the continuous presence of the tribe, and the necessity of having it continuously in mind, acts as an emollient of behaviour, creating adjustments superior to anything that prevails in any but the most highly civilized of our sort of society. How much of this accounts for the lessened operation of what goes with us by the name of "temperament," is difficult to say. Accepting Professor McDougall's definition of temperament as the "inner environment of character" produced by alterations of the chemistry of the body, one is obliged to report that while temperament is present in the primitive make up, it is much less obviously so than with us. One must suppose that alterations of the chemical constitution of primitives are less noticeable than with the so called civilized peoples. But whether that is a normal condition of primitiveness or a racial Amerindian characteristic, it is impossible for the writer to say.

Accepting personality as the impact upon the observer of the totality of all its distinguishing factors, as described by Professor McDougall, namely, intellect, character, temperament, disposition, and temper, one cannot deny it to the tribesmen, but one admits that the least salient of these are intellect and temperament, and that character, that is to say the drive and direction of personal traits, is the most outstanding. And of this drive and direction it can almost invariably be said that they are the product of explicit disciplines, which lie along the path of the genius emergence. Tell me the things an Indian does most naturally, subconsciously, without instruction, and I will be able to make a tolerable guess at his character, because I will know to what discipline he most naturally submits himself.

MORPHOLOGY AND CHARACTER

E. LANDAU
Lausanne

The question as to whether certain attributes of character and certain talents or endowments correspond to certain physical qualities, is one that must interest not only the scientist, but also the layman.

Can we, from the outward form of the human body, or of this or the other part of the body, draw any conclusions regarding the psychic aptitudes? Many attempts have been made toward the solution of this problem.

Lavater believed that it was possible to predicate the inner value of a man from his features. We know to what lengths he went in his fantastic physiognomical studies, and how little positive result they furnished. The phrenological doctrine of Franz Gall was not much more successful, although here a more authentic material basis was available—namely, the assumption that the cerebral cortex may in one way or another affect the external configuration of the skull, so that one may deduce, from the form of the skull, the greater or lesser development of this or that portion of the brain. The admirable researches of G. Schwalbe have verified the existence of regions in the skull where the external configuration is actually moulded by the brain and therefore reproduces the outward form of the brain. In practice, however, phrenology has accomplished little; for one thing, most of the localities described by Schwalbe are covered with muscles in the living subject; for another, the localization of function in the brain itself is far from being so advanced that we can diagnose a given aptitude or quality of character from a given protuberance of the brain—and hence, from a given formation of the surface of the skull.

The somatic types of Siguad (muscular, pneumatic, digestive, and cerebral) acquired an interesting characterological significance when it occurred to Kretschmer to inquire whether the various

physical types might not be brought into closer relation with the incidence of certain psychoses. The results of his investigations proved that his assumption was valid in principle. On a basis of anthropological and radiological research the present writer has succeeded in differentiating (for example) between the coarse and the delicate skeletal types. The slender and graceful skeletal man is likewise the physically active man, fond of dancing and athletic sports, whereas the coarse and heavy skeletal type is commonly more quietly deliberative, and slower to act.

With the sudden development of the comparative anatomy of the central nervous system investigators endeavoured to inquire more precisely as to whether it would not be possible, by comparative anatomical research, to determine what particularities of the human brain were to be regarded as characteristically human. So long as their investigations were confined to determining the cerebral typology of the primates they were brilliantly successful. They were able to show that the brain of the primate possesses the absolutely and relatively largest fore brain, the relatively largest cerebral cortex, and the most fully developed insula; and that the brain of the primate is a microcosmatic brain. While in the reptiles almost the whole exterior surface of the forebrain belongs to the olfactory lobe, in the higher animal forms (for example, in the carnivora) the olfactory lobe (the apparatus of equal importance for the capture of food and for the mating instinct) is found at the base of the forebrain.

In the primates the olfactory lobe, as Edinger and his pupils have shown, is entirely involved in the interior surface. In the primates the cortex of the forebrain, as Ch. von Jakob has shown, is rotated about the transverse axis of the forebrain, a rotation which gives rise to the insula, inasmuch as the insular cortex is left unmoved at the pole of the axis of rotation. The present writer has been able to show that we have here not simply a mere rotation, but an actual bending of the forebrain, including the olfactory lobe. Consequently a characteristic of the primate is *a greatly developed cortex of the anterior lobes and a consequently diminished development of the olfactory lobe.* The human cerebrum is larger and its convolutions more complex than those of the anthropoid apes. But the hopes entertained by certain investigators, that it would be possible from the external form of the human cerebrum to draw char-

acterological or psychological conclusions in respect to the owner of the brain in question, have been shown, by Poynter's investigation of criminals and Strida's examination of the brain of a man very highly endowed in one particular direction, to be illusory.

Now research was directed to the cellular architecture of the cerebrum—that is, to the study of the stratification of the nerve-cells in the cortex. This stratification is not everywhere the same, and in so far as one accepts the sixfold stratification proposed by Brodmann as the basis for the comparison of different cyto-architectonic areas, we can today with C. von Economo, differentiate over one hundred areas of the cerebrum (apart from the olfactory lobe). The six strata proposed by Brodmann as the basis of all cyto-architectonics are: the lamina zonalis, the lamina granularis, the lamina pyramidalis, the lamina granularis interna, the lamina ganglionaris, and the lamina multiformis. Accordingly as one or the other of these strata is thicker or thinner, accordingly as the nerve-cells are larger or smaller and more loosely or closely packed, new areas are constantly being described by research-workers. While the third, fifth, and sixth strata consist principally of comparatively large, pyramidal, or possibly spindle-shaped nerve-cells, the second and fourth strata consist of very small granular cells which on closer investigation prove to be also very small nerve-cells. And it is precisely to these very minute cells that recent investigators ascribe the function of receiving, elaborating, and forwarding the stimuli reaching the cerebrum from the thalamencephalon and mesencephalon. The strata containing the larger pyramidal cells, on the other hand, are concerned mainly with the motor functions. The granular layers are above all sensory strata, and the cortical areas in which the granular strata are largely expanded at the expense of the pyramidal and spindle-shaped cells are par excellence receptory-sensory regions of the cortex. On account of the predominance of the very minute cells, von Economo has described these regions as the koniocortex (dust-cortex). We shall find the koniocortex wherever sensory-representational areas are localized in the cerebral cortex.

Those regions likewise are rich in the very minute elements of the inner granular layer in which Flechsig localized his two great association-centres: namely, the region of the frontal lobes, and the region of the inferior parietal lobes. The extremely interesting researches of C. von Economo have on the whole confirmed Flechsig's

hypotheses, and he too believes that in the second frontal convolution and the lower parietal fold we have two centres which must be of predominant importance for the expression of the psychic and intellectual personality. Flechsig's and Economo's theory that the progressive cerebration of the human brain must have been manifested mainly at these two points tends strongly to support the Jakob-Landau theory of a progressive humanization of the fore-brain by a rotation or bending of the cerebral cortex about the region of the insula (and about the basal ganglia deeply embedded in this region).

From the above remarks it might be supposed that we are successfully approaching a localization of the human faculties, and above all of the attributes of human character, in the cerebral cortex. But it was precisely the clinico-neurological researches of von Economo into the so-called *Encephalitis lethargica* (sleepy sickness) that, as he himself has said, seems in some degree to dethrone the cortex from its predominant position, and, in accordance with the doctrines of elder investigators, to make certain important characterological qualities—such as will, initiative, affective excitability—dependent, in a psycho-motor sense, on the basal ganglia of the fore-brain. But even the optic lobes and the ganglia situated beneath them, as well as the socalled central ventricular grey matter, may play some part in these manifestations of character.

These regions will of course be influenced not merely by nervous stimuli, but will also be subject to the influences of the glands with intermost reactions, and various hormonal influences.

Thus we see that it is hardly possible to draw characterological conclusions merely on the basis of cerebro-anatomical or histological researches. We must always keep the chemical aspect of the mental processes in mind.

And so, once more, we have a circle of reciprocal influence between the central nervous system, the sympathetic and parasympathetic systems, and the glands with internal secretions. The nerves stimulate the glands and thereby incite them to increased activity; the secretions of the glands or hormones exert their influence in turn on this or the other part of the nervous system.

Received January 3, 1932.

NEWS AND NOTES

Germany

The Ninety-second Meeting of the Society of German Scientists [*Naturforscher*] and Physicians at Wiesbaden and Mainz brought a vast amount of information. Herr Brasch of Berlin read a paper on his latest experiments in disintegrating the atom. He has succeeded in disintegrating the atom of a heavy element like lead. Such considerations as yet belong largely to the theoretical sphere; at the same time the medical effects of disintegrating the atom are already of considerable importance. Brasch has succeeded in producing radium rays of sufficient strength to cure cancer in mice.—Herr Zondek, whose main sphere is the study of hormones, reported on the sexual female hormone called by him "prolan." It is already possible to discover pregnancy on the second or third day. —Herr Spemann (Freiburg) brought valuable contributions on recent results of the study of embryology. His subject was "Experimental Contributions to the Theory of Embryology." Herr Spemann has observed the development of the fertilized ovum in the very earliest stages. Thus small sections removed from various regions of an amphibious ovum were interchanged with others removed from different regions. In certain circumstances that which later becomes the skin of the back can contribute to the structure of the brain if it is implanted in the appropriate region. Spemann has already succeeded in interchanging tissues of fertilized ova of different animals. In this way it is possible to form organs not properly belonging to the normal type. If, e.g., a piece of skin from the belly of a frog embryo at a very early stage is implanted into what will later be the mouth of a newt, a mouth instead of a piece of belly skin is formed in its place. The mouth, however, will be that of a frog instead of a newt. It follows that the germ is not a "machine," but is, so to speak, reasonable in its behaviour.

* * *

The Central Institute for Education and Instruction was asked by the Prussian Ministry of Education to cause teachers of all schools to deal with "Theory of Heredity and Treatment of Heredity." The meeting took place on October 11-13 at the Central Institute, and was very fully attended. There were numerous lectures delivered by eminent authorities. We are unfortunately compelled to confine ourselves to that part of the proceedings immediately connected with the subject of the present periodical.

Herr Eugene Fischer (Berlin) spoke on: "An Introduction to the Theory of Human Heredity." Whereas with animals and plants the student is generally in a position to observe numerous generations, the anthropologist is restricted to a small number. The small number of descendants also adds an element of confusion. The study of twins, nowadays a sovereign method for the study of heredity, has certain advantages over the experimental study of heredity in plants and animals. Naturally the influence of environment on the individual is of great importance; epigrammatically we may say: "Heredity provides what man may become, environment what he does become."

Freiherr von Verschuer spoke on

the theory of heredity and the study of twins. His lecture was complementary to that of Herr Fischer. The study of twins is a most valuable means in the study of human heredity; its results supplement those of the study of families, which latter is not very extensive. By this means the influence of heredity and environment can be delimited in monoval and bi-oval twins, and the width of the sphere of reaction of hereditary dispositions can be determined fairly definitely. Whereas monoval twins can be treated as equal from the point of view of heredity, bi-oval twins simply have similar hereditary dispositions such as may also be found in brothers and sisters not twins. Monoval twins (who are always of the same sex) clearly show the influences of environment. The study of twins has shown that intellectual and psychic properties form part of heredity as well as physical disposition and characteristics. This lecture—which, like that of Herr Fischer, was illustrated by photographs—culminated in the words: "Hereditary disposition may be a fate, but may also imply a duty."

Scandinavia

The Fifth Congress of Scandinavian Psychiatrists

About 130 psychiatrists from Sweden, Norway, Finland, Iceland, and Denmark met at the *V. Nordiske psykiatriske Kongres* in Copenhagen from August 23 to 26, 1932. The main theme was: "The etiology and pathogenesis of endogenous psychoses." The dominating point of view was the organic-materialistic, an almost endemic feature in the average Scandinavian psychiatrist, constituting a one-sidedness which unavoidably will detract from the value of even the organic-materialistic results themselves.

One of the most interesting contributions was given by Helweg (Denmark), "Artistic Production by Schizophrenics," which was followed by a very instructive demonstration of drawings and sculptures made by a patient in the Oringe Asylum.

It was shown that the patient who in a more normal mental state had been able to produce only rather common and uninteresting works of art, during the bad (schizophrenic) periods rose to an exceedingly high artistic level of strikingly "modern" originality.

Tomasson (Iceland) lectured on "The Treatment of the Manic Depressive Depression" and gave several valuable hints as to the various remedies available. His hypotheses were founded on the assumption that the vegetative nervous system is the medium through which psyche and soma meet and collaborate.

German psychiatry was represented at this otherwise purely Scandinavian congress by Oesterreicher who read a paper on "Hormonal Research on Endogenous Psychoses."

The contribution of Wigert (Sweden), "Methods of Anthropometry," relied, naturally, upon the almost epochal researches of Kretschmer. Generally speaking the Scandinavian psychiatrists have with very much greater readiness taken to the ideas of Kretschmer than to those of Freud. The explanation of this curious difference in understanding and appreciation is to be sought (and found) in the circumstance that Kretschmer always takes the somatic factor, the shape and build of the body so strongly into consideration. The weighings, measurements, etc., hereby necessitated, and the other so called "exact" methods, are things to which the Scandinavian doctors because of their almost purely organic-materialistic training are accustomed; it is no wonder that they have "reconciled" themselves to Kretschmers' imagination and the bold genius of his ideas.

Brünnicke (Denmark) demonstrated the Danish State Asylum,

where such criminals as cannot be condemned according to the General Criminal Law, are detained. Castration has been carried out on several of these "criminal patients" but only if they themselves (together with their spokesmen) agreed to the operation and if the crime was violent sexual attacks on children or very young girls.

The result brought about a diminishing of the libido sexualist and the criminal impulses. Brünnicke read aloud some almost pathetic letters of gratitude, received from earlier patients who now get on fairly well, untroubled by temptations of any kind.

As the sole defender of a more purely psychological point of view the present correspondent read a paper on "Cinema-play as Psychotraumatic Factor." A case was described where a patient who for years had suffered from a morbid dread of dwarfs and hunchbacks was completely cured by psychoanalysis. During the treatment it was made clear that the symptoms originated in the harmful mental effect of an especially "ghastly" cinema play which the patient happened to witness at the age of six or seven years. Instead of names such as "Phobia," "Anxiety-Neurosis," etc., the lecturer suggested the terms Psychotraumatic-neuroses for such ailments.

OLUF BRÜEL, Copenhagen.

BOOK REVIEWS

THE ENERGIES OF MEN. A Study of the Fundamentals of Dynamic Psychology. By William McDougall. New York, Scribners; London, Methuen & Co., 8/6. Pp. ix, 389.

Professor McDougall's *Energies of Men* is a book for which many teachers, who balked at requiring their students to read both his *Outline of Psychology* and *Outline of Abnormal Psychology* in their elementary courses, have been eagerly waiting. *Energies of Men,* which is reminiscent, in name, of an inspired essay by William James, is not only an epitome of Professor McDougall's psychological views up to 1932; it also brings fresh material to bear on many subjects and, in a sense, it may be regarded as the 1932 analogue of James's justly famous "Briefer Course."

Unlike many recent textbooks, it deals not only with facts and dogmas but with *problems* and *foundations.* Here are passed in review all the conscious and behavior phenomena of man and beast in the systematic manner and well-knit order characteristic of the author. There is very little deviation from his former position in any of the theories expounded, but the tone is more judicial and tolerant of other schools. The abridgement of his larger works is remarkably proportional throughout, although at times one feels that in some connections a longer discussion would be helpful. One instance of this is in the application of the all-or-none principle to the issue in question on page 333. The most important chapters in the book, because of their treatment of recent research, are those on the fundamentals of the physiology of the brain, on the organization of the affective life and on learning and the steering processes. The appendix on the process of conditioning (chapter XIII) is particularly illuminating.

On the other hand, the chapter on schools of psychology does not seem to be adequate, in view of the many existing "psychologies"; and conversely, if Spearman's factor doctrine is to be regarded as a separate school, then at least a score of other points of view bearing on a restricted department of the science may justifiably be included. At the same time branches of psychology, like physiological psychology and philosophical conceptions like epiphenomenalism are all treated under the head of schools.

The frequent use of the term innate *propensities* for instinctive urges sounds somewhat like an apology and rather smacks of the phrenological nomenclature, although of course no particular objection may be cited against employing such a term. Propensity, in this case, most likely corresponds to the more traditional term "tendency."

In the chapter on "Disposition, Temper, and Temperament," we are told that "a man's disposition is then the sum total of his propensities" (p. 171). Now if by propensities we are to understand constitutional or innate tendencies, as the author implies, then apparently a disposition is the totality of instincts in any given individual or the particular instinctive blend perhaps. But surely this is not what is meant by disposition in ordinary parlance. Furthermore, in a later chapter we find the term "disposition" with the qualified "cognitive," as a synonym of "ideas."

> We distinguished dispositions of two principal classes; on the one hand, the various abilities, each of which is so constituted as to per-

> form, when set in action, some special function; on the other hand, the propensities, which we likened to so many dynamos, each of which can generate energy and supply it to various abilities, thus activating them or bringing them into play. (p. 318)

Now, this dual, or even perhaps threefold, application, in a sense, is apt to confuse the reader.

Nor is the meaning of the term "temper" quite clear in Professor McDougall's theory of personality. In daily conversation, we understand what it is to "have a temper" or "to lose one's temper." It is a show or outburst of anger. In the same sphere, to speak of an even temper, a hasty temper or an ill-temper is to refer to the relative quickness with which the emotion of anger is aroused as well as its intensity, but to make of temper a major category of personality and to add such types of temper as "steadfast" or "persistent," "urgent," "energetic," "mercurial" appears to be making a great deal of a linguistic wrinkle peculiar to the English language.

As a matter of fact, the word temper is a popular abridgement of the more technical word temperament; and if temper has come to mean an outburst of anger, it is because temperament too has the same meaning in non-technical language, e. g. in the phrase a "temperamental" person. Neither in German nor in French is there a word doing duty for the category which temper, according to Prof. McDougall, purports to represent. It is either *Laune* or *Stimmung* or *Gemütszustand* in German, and in French either *humeur, disposition, constitution,* or *tempérament.* Now, if we were dealing with a real fundamental of personality and not merely a trait with variants, there surely would be a separate term for it in either French or German, if not in both languages.

Instead of distinguishing disposition, temperament, and temper along with character and intellect as "the five great classes" of factors in personality (p. 371) it would seem to be more to the point to merge disposition, temperament, and temper into affectivity, or in its broader sense, perhaps temperament; and add as a fourth fundamental, the physical qualities. In this way, we have as our four main categories of personality the following: the physical, the cognitive, the affective, and the volitional. The inclusion of tempers and dispositions would necessarily involve duplication, or at least overlapping. A choleric temperament implies a fiery or hasty temper. A sanguine temperament bespeaks a good-natured disposition. A disposition, as I should define it, is an attitude or approach toward others, particularly strangers, as manifested through the initial behaviour on meeting or getting acquainted. A crabbed person necessarily is possessed of a poor or ill-natured disposition. A kindly disposition is one which shows evidence of friendliness and helpfulness. A sunny disposition is another name for a permanent cheerful frame of mind.

The chapter on sentiments is thus far the clearest exposition of this important doctrine, and although there is much that I cannot subscribe to here, it will be best to reserve the discussion to a more appropriate occasion than a review.

The language of the book is relatively simple and direct. Sometimes we even find a colloquial Americanism like "are up against" (p. 8) which the author's British colleagues will undoubtedly frown upon.

The omission of Watson's name in the book is probably an unstudied response to the excessive preoccupation with Watson in many recent books on psychology. The behaviourists have long ago adopted the policy of ignoring opponents.

The book almost symbolically ends with a discussion of courage, the true type of which is invested with an intellectual factor, a view which many will agree with. Less convincing is the codicil, "If anything could add to

his courage it would be a firm and reasonably grounded religious belief, a belief that, so long as he acts according to his best light, he has, in an ultimate sense, nothing to fear," unless of course that religious belief is interpreted with great latitude so as to include the courage of a Spinoza or even the courage of an atheist like Bob Ingersoll, Brann, and Thomas Paine."

In conclusion let me predict that Professor McDougall's *Energies of Men* will be a welcome addition to the libraries both of those institutions where the mechanistic and behaviouristic view of life has captivated the psychological teaching staff as well as of those colleges which have been able to resist the onslaughts of the oversimplified stimulus—response mindless psychology. It is a book which will stimulate thinking; and that is what the student needs above all.

A. A. ROBACK.

PROBLEMS OF POPULATION, BEING THE REPORT OF THE PROCEEDINGS OF THE ASSEMBLY OF THE INTERNATIONAL UNION FOR THE SCIENTIFIC INVESTIGATION OF POPULATION PROBLEMS. Edited by P. H. L. F. Pitt-Rivers. London: Allen & Unwin, 1932. 15/-.

The formation of this international union was the outcome of the World Population Conference held in Geneva in 1927. A year ago already twelve countries were represented in the Union by National Committees, while in five other countries such committees were in process of organization.

The present volume of proceedings is the first to be published, and contains the full text of all the papers read at the London meeting.

The chief factors which have coöperated in the variation of density of population are discussed, e.g. natural resources, geographical situation, political actions restricting the free movements from one country to another, falling birth rate accompanied by a falling death rate and therefore by a greater longevity, the problem of economic absorption of population, and the qualitative and quantitative distribution of the population.

The volume also contains the reports of some research commissions, and of national committees.

A central office for the general headquarters of the Union has been established, its address being c/o the Royal Geographical Society, London, S. W. 7.

THE POSSIBLE SELF. A Study in Religious Education as Adaptation. By Frank S. Hickman, Professor of the Psychology of Religion, Duke University. New York: The Abingdon Press, 1933. Pp. 123.

In America, where the religious neutrality of the state forbids teaching of religion in the public schools, responsibility for the development of character has been very generally laid upon the ministers of religion. And of recent years the responsibility has been further concentrated on the same shoulders by the breakdown of the family. No wonder, then, that, in face of the monstrous frequency of crime and delinquency, the ministers, well nigh in desperation, are looking to psychology for help in their heavy task. When they seek such help, they find themselves between the devil and the deep sea, between sheer mechanical materialism and freudianism, the only theories of human nature that enjoy a measure of authoritative esteem with the American public.

Professor Hickman's little book should earn him the gratitude of large numbers of such perplexed souls. For it is designed to introduce them to a theory of human nature which is well suited to their needs, and is, although its immediate ancestry is British rather than American, of respectable antecedents (it can, in

fact, claim Aristotle as the founder of its line) and is supported by a very reputable body of academic authority. The theory in question, now beginning to be known in academic circles under the name 'hormic psychology' is well suited to the needs of those for whose use it is designed, well suited in two ways. First, it is not inconsistent with the moral or the religious convictions of any enlightened minister; for it teaches that man is not merely a machine subject to strictly mechanical determinations, but a being capable of being moulded to spiritual ends by the benign influences of a lofty tradition and of being stimulated to aspiration and effective effort for the realization in himself of an ideal of personality.

Secondly, the theory is well suited to the needs of the character-guide, in that it gives an intelligible analytic account of that something within a man which has long been recognized as the source of all resolute and consistent conduct of the higher kind and which yet has been known only in its expressions in action and not at all in its own nature; and it provides also, at least in outline, an account of the stages of growth and progressive integration by which the impulsive and passionate nature of the child passes over into the disciplined character of the adult, the man capable of self-control, of choosing between good and evil, and of sustained effort towards the chosen goal.

All this, together with the principles of application to the problem of character training, is admirably set forth by Professor Hickman in language as clear and simple as the complexity of his topic permits and with the aid of skilfully used illustrations. The book may be warmly recommended as a successful and much needed effort in the popularization of a scientific theory of far reaching significance for the most vital problem of our troubled age.

W. McDOUGALL.

KREUGER. By Poul Bjerre. Natur och Kultur, Stockholm. Martin, Copenhagen. Pp. 383.

This is a psychological biography of the late Swedish financier and trust president, Ivar Kreuger, of world-wide although now herostratic fame. He took his own life in Paris immediately before the complete break-down of his vast affairs and thus escaped the judicial sequel to a financial catastrophe, the effects of which were to be felt in most countries of the world.

The author, the well-known Swedish psychoanalyst, in this psychopathological biography, especially draws attention to Kreuger's marked *deficiency in checking impulses,* and sees in this trait the key to Kreuger's character, his success and his downfall as well.

In fact, the danger begins first when Kreuger leaves the secure footing of a big financial trust for the Utopia of a world-mastery of the match industry. As Napoleon before him, he could not keep back from the alluring, although fateful, step from fact to fancy; and both had to pay their price.

The author does not seem to see, however, the utter importance of Kreuger's love affair with the Norwegian girl he met in his youth. This occurrence appears to me so isolated, so curiously out of accord with Kreuger's psychology and later mode of living, that it must be regarded as a *psycho-traumatic agency* of no mean determinating power—very likely establishing the turning-point in a career which would otherwise have been, perhaps, less brilliant, but on the other hand free from the devastating touch of pathological criminality.

OLUF BRÜEL,
Copenhagen.